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THE

TRANSYLVANIA JOURNAL

OF

MEDICINE

AND THE ASSOCIATE SCIENCES.

EDITED BY

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AND

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VOL. IV.

LEXINGTON, KENTUCKY.

PRINTED BY JOHN F. HERNDON & CO.

1831.

148359

NOTICES.

Since our last acknowledgement of exchanges we have received the following works; viz.

1. The American Journal of the Medical Sciences, Nos. XVI, XVII, August and November 1831.
2. The North American Medical and Surgical Journal, Nos. XXIII, XXIV, July and October 1831.
3. The American Journal of Science and Arts, conducted by Benjamin Silliman No. 2, vol. XX. Nos, 1 and 2, vol. XXI.
4. The Journal of Health, conducted by an Association of Physicians in Philadelphia Nos. 21 to 24, vol. II. and Nos. 1 to 9, vol. III.
5. The Boston Medical and Surgical Journal, (weekly) Nos. 20, to 26, vol. IV, and Nos. 1 to 20, vol. V.
6. The New York Medical Journal, No. IV.
7. The Western Journal of the Medical and Physical Sciences, Nos. 1, and 2, vol. V. July and October 1831.
8. The Maryland Medical Recorder, Nos. 2. and 3, vol. II.
9. American Annals of Education and Instruction, and Journal of Literary Institutions; conducted by William C. Woodbridge, Boston: Nos. 7 to 12 inclusive, vol. I. and No. 1, vol. II.
10. The monthly American Journal of Geology and Natural Science; exhibiting the present state and progress of knowledge, in Zoology, Botany, Mineralogy, Comparative Anatomy, Chemistry, Meteorology, Physical Natural Agents, and the antiquities and Languages of the Indians of this Continent, conducted by G. W. Featherstonhaugh, Philadelphia: Nos. 1, to 7, inclusive, vol. I.
11. Select Medico-Chirurgical Transactions, vol. 1. A collection of the most valuable memoirs read to the Medico-Chirurgical societies of London and Edinburgh; the Association of Fellows and Licentiates of the King and Queen's College of Physicians in Ire-

land; the Royal Academy of Medicine of Paris; the Royal Societies of London and Edinburgh; the Royal Academy of Turin; the Medical and the Anatomical Societies of Paris, &c. &c. &c. Edited by Isaac Hays, M. D. Editor of the American Journal of the Medical Sciences, etc. etc. (From the Publishers Messrs. Carey and Hart, Philadelphia.)

12. Porter's Health Almanac for the year 1832. Calculated generally for all parts of the United States. The maxims and rules for the preservation of Health, under the direction of the Editors of the Journal of Health. (From the Publisher.)

NEW WEEKLY MEDICAL JOURNAL.

H. H. Porter, *Literary Rooms*, 121 *Chesnut Street, Philadelphia*, proposes to publish in the first week of January 1832, a new Medical Journal, to be called the *Journal of Medicine.*

It will be published weekly—each number to consist of eight quarto pages.—Terms $2 50 *per annum in advance. Postage the same as that on newspapers.*

Care will be taken to make the Journal of Medicine a record of the current medical literature of the day, by editorial notices, critical analyses, and reviews—as also of clinical medicine, by details of cases in hospitals and private practice; with an especial attention to all improvements and discoveries in therapeutics. Needful reforms in the present modes of medical instruction; and ameliorations required in the ethics and practice of the profession, will be freely suggested. As forming a part of the history of medicine, biographical sketches of distinguished physicians, surgeons, &c. will obtain a place in the columns of the Journal.

Errata.

The reader will please to notice the following errors in this number, viz.

Page 528, for Unio *capillus* read U. *lapillus.*

" " 13th line from bottom, for *tooth* read *teeth.*

" " 2d " " " for *spine* read *spire* and the same on page 529, 2d line from top.

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NOTICES.

Since the publication of our last number, we have received,

1. The American Journal of the Medical Sciences, No. XIV. (In Exchange.)

2. The North American Medical and Surgical Journal, No. XXI. January, 1831. (In Exchange.)

3. The American Journal of Science and Arts, conducted by Benjamin Silliman, M. D L. L. D. &c. No. II. Vol. XIX. (In Exchange.)

4. The Journal of Health, conducted by an Association of Physicians in Philadelphia, twice a month; Nos. 6 to 13, Vol. II. inclusive. (In Exchange.)

5. The Journal of Law, conducted by an Association of Members of the Bar in Philadelphia, twice a month; Nos. 11 to 17, inclusive. (In Exchange.)

6. The American Lancet, semi monthly; Nos. 1 to 4, Vol. I. New Series. (In Exchange.)

7. The Illinois Monthly Magazine, conducted by James Hall. Nos. 4 and 5, Vol. I. (In Exchange.)

8. The Boston Medical and Surgical Journal, weekly; Nos. 1, 2, 3, 4 and 5 Vol. IV. (In Exchange.)

9. The New-York Medical Journal, No. I. November, 1830; conducted by Daniel L. M. Peixotto, M. D., John R. Rhinelander, M. D., John James Graves, M. D. (In Exchange.)

10. American Annals of Education and Instruction, and Journal of Literary Institutions; conducted by William C. Woodbridge, Boston; Nos. 1, 2, 3; Vol. I. monthly. (In Exchange.)

11. An Introductory Lecture, delivered at the College of Physicians and Surgeons of the City of New-York, Nov. 5, 1830. By John B. Beck, M. D., Professor of Materia Medica and Medical Jurisprudence, in the University of the state of N. Y. (From the Author.)

12. Catalogue of the Officers and Students of the University of Pennsylvania. Philadelphia, January, 1831.

13. Annual Catalogue and Circular of the Medical College of Ohio: during its Session of 1830–31, Cincinnati.

In discontinuing the publication of the "*Essays on the Absorbent System and its Functions*," we consider it due to the author to say, that the discontinuance has proceeded solely from a wish on the part of the Editors to give a greater variety to their Journal than comported with the prolonged and continued appearance of so lengthy and elaborate an Essay. We are pleased to inform our readers that the author proposes to republish it at length in a separate form.

The distant point from which we are compelled to procure our supplies of printing paper, has subjected us to frequent disappointment and delays, and to obviate the inconvenience as far as possible, it will be perceived by the cover of this number of the Journal, that the periods of its publication have been in some measure changed; and in future it will be issued at any convenient time within the quarter. This arrangement will enable us to lay before our readers some of the more important matter of our cotemporaries which may appear within the same quarter.

THE

TRANSYLVANIA JOURNAL

OF

MEDICINE

AND THE ASSOCIATE SCIENCES.

FEBRUARY, 1831.

ORIGINAL COMMUNICATIONS.

ARTICLE I.—*Practical Observations on Simple and Congestive Fevers.* By JOHN H. SAVAGE M. D. of Jefferson county, Mississippi.

IN a young physician who is just entering upon the field of practice, with no light to guide him but the experience and authority of his predecessors, it would require no common share of penetration to pursue his course with an unfaltering and undeviating step amidst the confusion and uncertainty which too frequently surround him. The contrariety of opinion which he often meets with in different writers upon the same disease, serves only to bewilder his mind, and to involve the subject in deeper intricacy; while his practice founded upon such incongruous principles is liable to be marked with inconsistency and a want of energy. In simple and congestive fevers there is a great want of uniformity among physicians in relation to the best treatment to be pursued in them. By some, the lancet is regarded as the Ægis of safety, by others it is viewed as a doubtful and even a dangerous remedy; by some the free

administration of purgatives is considered as an indispensable part of the treatment of all febrile diseases, while another class of physicians believe them unnecessary and pernicious, and only calculated to increase the evil which they were intended to remove; again, some believe tonics entitled to a high rank among the most prompt and effectual remedies in these diseases, while others with equal confidence believe them often productive of the most serious and fatal results.

In searching for the causes of such a singular disagreement, we cannot but believe, that an inattention to the diathesis of disease and the varying states of the system during its progress, has had its full share in producing such conflicting views in regard to practice. The great object in the treatment of all diseases is to restore the normal functions of the disordered organs; but the plan of treatment which would be successful in a disease of one diathesis, might be fatal in another, although the disease may be nosologically the same. In some years our diseases will exhibit an inflammatory character, in others they will be marked by a state of simple excitement, and lastly they may be wholly or partially of a congestive character. Without alluding particularly to the various opinions, all of them hypothetical, which have been assigned as the causes of this unsteady and mutable aspect of disease, it will be sufficient to remark, that it is dependent upon some peculiar and inexplicable changes in the constitution of the atmosphere. It is on this principle that the axiom of the illustrious Rush that "all diseases wear the livery of great epidemics" is to be explained. The same occult but all pervading temperament of the atmosphere which invests an epidemic with a peculiar form and type, communicates the same type to all subordinate diseases. For some years past, our diseases have scarcely partaken of an inflammatory character; but for the most part have made their appearance under a state of simple excitement or of congestion; and it is to these fevers that we are now about to direct our attention. As we intend our remarks in the following paper to be entirely of a practical nature, we shall endeavour to encumber it with as little unnecessary detail of symptoms and

other particulars, with which we are all familiar, as is consistent with a perspicuous and connected arrangement.

FEVER OF SIMPLE EXCITEMENT.

General remarks—Since our diseases have lost the inflammatory diathesis by which they were characterized several years ago, they have, as I remarked a moment since, generally assumed a form of simple excitement with the exception of two or three years past in which we have been visited by congestive fever, either as a sporadic or endemic disease.—This state of excitement, however, has varied in different years; now rising almost to inflammatory action, then verging to a state but little above congestion. It differs too of course in different individuals according to their various constitutional vigor. The principal symptoms which belong to this fever, are, a chill succeeded by reaction, foul tongue, pain in the head and back, pulse sometimes full, soft and frequent, sometimes hard, considerable thirst, dry skin, heat universally diffused, &c. Dr. Armstrong has endeavoured to point out the difference between an inflammatory disease and one of simple excitement, and is inclined to the opinion that in the former, there is a greater topical accumulation. But the difference seems to me to consist merely in a minor degree of reaction in the system in the latter disease, and of course a less degree of constitutional tone at the time of its invasion.

This fever when it occurs in Southern latitudes and is associated with bilious symptoms, is supposed by many to be the exclusive product of marsh miasm. An argument against this idea which occurs to us, is that as a mitigated form of miasm could alone produce a disease of moderate excitement, so those who enjoy a high degree of tone and health, should by the inherent powers of the system, resist the influence of this miasm. The disease, of course, should always be confined to the aged, the infirm and the young. This form of fever, therefore, would seem to derive its peculiarity as much from some constitutional inquination of the atmosphere, as the inflammatory or congestive.

TREATMENT—As I intend to enter somewhat at large upon the treatment of congestive fever, much of which will apply to the fever at present under consideration, I shall now confine myself to such points of practice, as seem peculiarly to belong to it.

Bleeding.—In general, it will not be found necessary to abstract blood freely in this fever, unless the degree of excitement prevailing approximates closely to inflammatory action; which will be indicated by the tenseness of the pulse and the violence of the symptoms generally. In individuals also of great constitutional vigor, this evacuation by reducing the excitement will promote a more speedy restoration of the secretions to their healthy state.

Purging.—Although this fever would seem to possess but little difficulty of management, yet where the degree of excitement is but little above the healthy standard, an inattentive or unskilful practitioner may readily convert it into a congestive form of disease by a little imprudence or precipitancy. A judicious course of purgatives, consisting of calomel and its usual combinations and adjuvants, and administered so as to keep up a slow but steady operation on the bowels, constitutes most certainly an important part of our treatment; and this should be continued as long as the dejections exhibit a vitiated state of the biliary secretion. But on the other hand, it is often not difficult to carry the operation of purgatives to a pernicious and dangerous extent. Too little excitement is as unfriendly to the healthy operation of the various functions as too much. The general system may be so much exhausted by the abstraction of an unnecessary quantity of blood, or the tone of the glandular system may be so much impaired and overcome by excessive alvine evacuations as entirely to suspend its secretions. In this way, I have seen fevers of simple excitement converted into congestive, with all their train of alarming and distressing symptoms, their want of secretion, their unequal excitement, oppression, &c. In some persons, in whom there is combined great irratibility with rather a deficient constitutional energy, the tone of the glandular system is easily ex-

hausted and the disease undergoes the change just spoken of. A friend of mine was for several years subject to an annual attack of fever of simple excitement. Some purgative medicine was usually promptly administered which at first brought away bilious discharges, which soon became frequent and watery and afterwards rapidly assumed a sanguineous appearance. The strength now failed, the skin became cold, and all the symptoms of great congestion occurred. I have been called to see him two or three times in this situation. Blisters were generally applied to the abdomen, some laudanum administered to check the discharges from the bowels, and a little whiskey or brandy was given to reanimate the prostrated powers of the system. During the past summer three cases of a remarkable tumefaction of the abdomen in children came under my notice which arose from hypercatharsis in this fever. Each had laboured under fever a week or ten days, and had been copiously purged with calomel and other medicines. Two of them at the time I saw them were taking medicines to expel worms. This tumid state of the abdomen was evidently caused by an accumulation of blood throughout the capillary system, which had lost its tone by an unnecessary perseverance in the use of purgatives. Dr. Miner remarks, that "tumid abdomen in fevers most probably is always caused by excessive purging, or spontaneous diarrhœa not seasonably checked."* This opinion is no doubt correct as regards the fever under consideration and others of weak action; but in puerperal fever, the distension of the abdomen depends upon "the swelling of the cellular tissue subjacent to the peritoneum,"† and is generally to be removed only by bleeding and free purging. The difference is, that in the former disease the capillaries are in an atonic state, in the latter in a tonic. The cases to which I have referred were relieved by large blisters in order to restore the equilibrium between the internal and external capillaries, by administering stimulating drinks and suspending the further use of purgatives.

* Vide Medical Recorder, Miner on Typhus Syncopalis—No. 39, page 217.
† Vide Bichat on Pathology—page 54.

There is another point in relation to the use of purgative medicine which should not be overlooked—the doses of medicines of this class should be in exact relation to the degree of excitement or tone in the system to be operated upon. I have often observed that where a small dose of purgative medicine would probably have operated kindly and beneficially, the object has been entirely defeated by the patient's voluntarily taking, or sometimes by my own direction, a dose so large as to be out of proportion to the tone then prevailing in the bowels and the glandular system. In consequence of this, the medicine would either not operate at all, or bring away scanty discharges of a watery nature.

Salivation—The importance of attending to the state of the system and the type of the disease in prescribing our remedies, is forcibly illustrated in the different, nay opposite effects, which a mercurial action exerts in fevers of different grades of excitement. The transcendent efficacy of a salivation in overcoming inflammatory diseases is known to every one conversant with medical writers; while its uncertain and often injurious effects in diseases of too little excitement are equally well established by experience. Salivation, therefore, will not only not cure this fever, but is often positively detrimental; and the reason appears to me obvious and unquestionable. Calomel as I just intimated seems particularly adapted to diseases of high excitement and in the early stage of sub-acute fevers. It is thought to cure fevers and other diseases by removing obstructions in the glandular system and causing an increased action in it; but this it does only by reducing the excitement to that state compatible with secretion. To secure its good effects in diseases of deficient excitement, it must be combined with such stimulants as will elevate organic sensibility; but in the fever under consideration this is unnecessary. It wears an excitement, a little higher than the healthy one, and in removing this by moderate purging, the secretions are re-established. I have no hesitation in declaring that every grain of calomel given after this object is attained is injurious. The tone of the glandular system is reduced below the state compatible

with secretion, and if any soreness of the mouth can be produced, it is what is called a dry salivation, and so far from being productive of any benefit, it will add to the debility, of course confirm the want of secretion, and will only accelerate the melancholy event which we are striving to prevent. "Even calomel," says Dr. Miner, "though the most certain agent during the first stages of regular fevers, in changing diseased action, if continued beyond the first week or even after the semi-critical change is produced, I am by no means speaking of chronical or local affections, is apt to *coincide* with and increase the general debility and irritability." If it be important to keep an eye upon the state of the system in order to secure the good effects of calomel, it is also important in order to obviate its troublesome effects. This subject appears to have been overlooked by Dr. Chapman, when he remarks, that "copious purging so far from checking the inordinate action of mercury, is the most certain mode of developing it."* This is certainly true when the inflammatory excitement is high, which free purging would so far reduce as to bring the system within the sphere of mercurial action; but a very different result would be realized from such a course, where the tone of the system was but little above or rather below the healthy standard. This reference is made merely to show that a circumstance so indispensable to be attended to in the management of disease, is too much neglected by many whose talents and whose writings are an ornament to our profession. It is from overlooking this very important subject, that we have been entertained with such various notions respecting the specific virtues of mercury and the relative powers of other medicines in the cure of syphilis. Sometimes the disease prevails during a period when the system enjoys full tone and excitement, and then mercury exhibits its most salutary effects; then again it makes its appearance under different circumstances when a mercurial action may require the aid of other medicines to eliminate the venereal virus.

* Vide Notes to Murray's Mat. Med. Vol. I, page 168.

Tonics—It is in this type of fever under its forms of Intermittent and Remittent that tonics are used with more success than in any other. In the first place, they are applicable to this fever of simple excitement, because there is nothing to forbid their use. The excitement is uniform, the circulation free and unobstructed, no organ oppressed, and the functions of the glandular system proceeding regularly. Secondly, Tonics are useful and often necessary on account of the readiness with which this fever passes, in some years, from a state of simple excitement to that of congestion. The period at which they are employed with the greatest advantage is where proper depletion has been resorted to in the first stage, and when, after dark bilious discharges have been effected from the bowels, they assume a more natural and healthy appearance. There is at this time a manifest subsidence of all the symptoms; the head is relieved, the tongue is moist and nearly clean, the skin recovers its soft and healthy state, and a perfect intermission or a very marked remission of the fever occurs. This is the point at which the powerful and happy effect of the sulphate of quina may be exhibited with the greatest advantage. The chain of morbid action is dissolved and the recovery of the patient is in general immediate and rapid. But if it be omitted and the physician still perseveres with mercurial or other purgatives, secretion becomes checked, a re-accumulation takes place in the portal circle, the balance of excitement is destroyed and the condition of the patient is greatly aggravated. Again, if when the crisis arrives to which I have just alluded. the careful physician suspends the administration of purgatives, and adopts an alterative course, or contents himself with simply keeping the bowels in a soluble state, the balance of excitement may be thus preserved, but the fever in all probability will maintain its hold. The energies of the system appear to be too much impaired to make an efficient and successful effort to restore health without the aid of tonics. It is not often, indeed, that any other plan of treatment will succeed: but I have known some cases where the evacuant treatment having been carried to the requisite extent and no farther, (that

of removing obstructions and restoring a healthy state of the secretions) the patient has fully recovered without the intervention of any other remedies. But this has generally been when the constitution was healthy, strong and vigorous, and the habits of the patient temperate and regular. But while a judicious use of tonics produces such favourable results in this fever, yet on the other hand a premature administration of them may lay the foundation of new diseases, or awake into action former predispositions formidable in their remote and ulterior consequences. Before a free secretion of bilious matter is obtained, the principle appears to me well established that the circulation through the abdominal viscera is obstructed; and if at this time a tonic be given, a reaction must ensue and local inflammations and chronic visceral enlargements must inevitably be the consequence. As cases of this kind are unfortunately too common, no difficulty would present itself in adducing the most abundant testimony in support of this assertion; but as a case of enteritis related by Dr. Cross in the 2d vol. and 2d number of this Journal, occurs at this time to my recollection and is in point, my friend will excuse the liberty of referring to it. The patient "was seized with a chill which was followed regularly by the stages of a paroxysm of intermittent fever"—an emetico-cathartic was administered which operated well, "after which the sulphate of quinine was given." "The paroxysm returned at 1 P. M. and lasted three hours. The quinine was again administered after its termination. Early on the following morning he took a very large dose which he thought made him worse." Dr. C. was called and found him with a tense full pulse; *sallow bilious skin and considerable abdominal pain which was much increased on pressure;* his stomach irritable; his bowels morbidly active, discharging copious *watery* stools." He was relieved by the administration of calomel and opium. These phenomena were dependent upon no idiosyncrasy of the patient as Dr. Cross supposes. It was the effect of the premature use of the tonic upon the principle before alluded to. A few cases of pulmonary disease emanating from the same erroneous practice have come within my knowledge; an

effect of the premature administration of bark and other tonics, which has long been known and commented upon by various distinguished medical writers. Sir John Pringle remarks, "though the paroxysms have disappeared under its use, (bark) yet having so often seen the *breast affected*, or a lurking fever remain after giving the febrifuge, at last I made it a rule to attempt the cure without it."*

The experience of the profession will fully confirm the testimony which is here adduced; and it should be sufficient to damp that overweening anxiety to perform hasty cures which induces some physicians to expose their patients to the danger of incurring chronic diseases. Sudden cures are from the very nature of disease seldom permanent, and chronic diseases are almost always more dangerous than acute. Perfect cures, then, should always be preferred to hasty ones; and the beaten track of experience to the many by-paths, which, though shorter, abound with dangers which lie concealed from our view. It is better

> ———"To bear the ills we have,
> "Than fly to others that we know not of."

CONGESTIVE FEVER.

Causes.—Some writers assign no other cause for the production of this fever than miasma of a concentrated and virulent character. This opinion derives its support, First, from the fact of its appearance late in the season, when malaria may be supposed to possess its most virulent activity. Secondly—By the greater comparative inability of the system to sustain itself under the effect of this miasm in consequence of the great waste of its energies during a long continuance of hot weather. These reasons are certainly not without weight, and are unquestionably calculated to give an obstinacy and severity to autumnal fevers which do not belong to those which occur at an earlier period of the season. But that this cause is inadequate to account for the production of this fever, and that some other cause is necessary to explain the peculiar diathesis of differ-

*Vide Rush's Pringle, page 183.

ent fevers will appear conclusively from the following facts: 1st. That sometimes congestive fevers do not appear for several years. Animal and vegetable decomposition may go on as rapidly as ever and the atmosphere may be impregnated with its poisonous offspring, the energies of the system may be expended by the alternate action of heat and moisture upon the surface, but still the fever of the season may not be congestive. The particular form of the fever whether intermittent or remittent may very probably be determined by the greater or less prevalence of the causes just mentioned, but we must look to another cause for the diathesis of the disease. 2ndly. Autumnal diseases are sometimes inflammatory, at other times they partake of a simple or sub-acute excitement, &c. If it were not for some peculiar temperament of the atmosphere, they would always be asthenic in their character, because debilitating causes acting upon debilitated persons must necessarily produce asthenic diseases. 3rdly. If diseases did not derive their diathesis from some peculiar temperament of the atmosphere they would never possess a uniformity of character. Every season would furnish us with a due proportion of inflammatory, of sub-acute and congestive fever, according to the tone of the system and to the greater or less degree in which local causes abound, and these diseases would prevail only in miasmatic regions. But what is the fact? In seasons in which there is this epidemic state of the atmosphere, as Sydenham terms it, districts of our country the most remote from miasmatic infection, the most remarkable for their general salubrity, are visited with severe and fatal diseases. 4thly. If miasm or other causes, as weather, changes of temperature, &c. were capable of controlling the diathesis of disease, we should expect to see it vary as the causes might differ in different neighbourhoods, in different states and in various parts of the continent. But epidemics which have spread over portions of our country, or have traversed from one end of the continent to the other, have always been marked with the same diathesis and reflected this peculiarity over all other diseases. This uniform character in the diathesis of disease is not confined, however,

to a period in which some great epidemic prevails. It may always be observed: when the diseases of the north are inflammatory, those of the south are so also; (making of course a due allowance for the effect of climate;) on the other hand, when typhus prevails at the north, congestive diseases belong to the south. 5thly. Mr. Webster in his learned and elaborate "History of Epidemics and Pestilential Diseases," has clearly traced a coincidence between epidemic diseases and the appearance of remarkable phenomena in the physical world, such as earthquakes, volcanic eruptions, comets, &c. And these last he has shown are almost always preceded or followed by singular irregularities in the seasons, &c.; and lastly he urges a belief that all these phenomena are but the effects of the universal operation of the electric fluid. It is reasonable therefore to presume, that with the great mutations in the physical world, animal life undergoes through the influence of the same causes, similar changes, which give to diseases their variable aspect. 6thly. The diathesis of diseases may be generally noticed to *change* with the appearance of epidemics—thus with the yellow fever of '93, the diseases everywhere became inflammatory; no doubt, in some places more, in others less so; and they continued inflammatory until the appearance of the pneumonia typhoides, since which, diseases have been losing that character.

From these facts, therefore, I conclude, First, that this epidemic quality of the atmosphere, whatever it be, and which prevails not only in particular seasons and years, but in a greater or less degree at all times and at all places, has the property of exalting or diminishing the energy of the nervous system. Secondly. When the nervous energy is thus augmented, the diseases are inflammatory; when diminished, they are congestive. Thirdly. It seems probable, that climate with its effects, prevailing weather, marsh miasm, changes of temperature, &c. have the power of determining the disease to particular organs, to the thoracic viscera in the north, to the abdominal viscera in the south.

Pathology.—Let us suppose, in conformity with my views of

the causes of fever, that such a condition of the atmosphere is prevailing as tends to increase the vital energies and exalt the constitutional powers of the system. Let us locate a person in a southern climate and imagine him surrounded by all the causes of disease which abound in low latitudes. Let us suppose that the biliary organs and some other parts of the secernent system have been for a long time unduly excited by the action of heat. Let us suppose farther, that he has been exposed to the combined enervating influence of marsh miasm, changes of temperature, &c. and what will be the necessary result? Indirect debility will follow, and local accumulations must take place in those organs which have been subjected to inordinate action from the causes just mentioned. What next? These accumulations become a source of irritation, and this impression is transmitted to the brain, and is thence radiated to the heart and arteries. The whole system in short sympathises with the local disturbance, and a high and powerful excitement is developed in proportion to the tone, or that specific quality which the system has acquired from the peculiar condition of the atmosphere. A bilious fever of strong inflammatory action is thus created, requiring free depletion to preserve the system from succumbing to the violent excitement under which it labors. In order to account for the developement of a fever of simple excitement, it is only necessary, that there should exist in the atmosphere a smaller portion of that principle which imparts to the system its increased vitality.

Let us turn our attention now to the consideration of congestive fever in connexion with the views which I have advanced in relation to its causes. The atmosphere is now destitute of that *pabulum* of excitement which elevates the natural tone of the system, and which in case of a disturbance of its normal functions, causes it to become the theatre of intense and powerful action. It no longer retains its stimulating qualities, heightening animal and organic sensibility. But let it not be supposed that Providence, in its bountiful provision for human existence, has consigned our lives to the uncertain support of an atmospheric principle which is liable to constant fluctuation.

The atmosphere is so composed as to be best suited for the purposes of life, and under all the vicissitudes of climate, of season and of situation, it remains the same. Although, therefore, the principle which I have spoken of as producing an inflammatory disease, may not be contained in the atmosphere, the various functions of life are still enabled to go on. But when in such a condition of things, a person is exposed to all the causes which impair the tone of the system, when he has become enervated at the close of a long and sultry summer, when marsh miasm has fully exerted its depressing influence, and when the dews of autumn and its cool winds are chilling the little remaining energy of the system, then he is in a suitable state to become the subject of congestive fever. But let us trace out in a brief and rapid manner the chain of disease through its various links. The congestion first occurs in the liver, because having been inordinately excited, it is now the first to feel the loss of tone; the irritation is transmitted to the brain, the impression of which transcends the enfeebled tone of that organ, and an accumulation takes place there. The brain ceases now to communicate its accustomed power to the intercostals and diaphragm, and the mechanical function of the lungs grows feeble, and congestion occurs there. The obstruction of the lungs now prevents the circulation of the blood from the right side of the heart, and an accumulation is the consequence there. The whole venous system becomes loaded with blood because the congestion of the lungs prevents a flow of this fluid from the right to to the aortic side of the heart, and the arteries discharge their contents into the veins by their inherent powers.

Such, then, is a very imperfect outline of my views of fever. The limits of an essay do not admit of such amplification, as the interesting nature of the subject would authorize; but at some future time, I may resume it in a more extended form.

TREATMENT.

Bleeding.—If the question were proposed to me whether bleeding be advantageous and necessary in congestive fever, a decisive answer must depend on the circumstances of the case.

If in inflammatory fever bloodletting be required to moderate the violence of arterial action, and if the state of the system in congestive fever be the reverse of this, it would follow as a natural inference, that the abstraction of blood in this latter disease could not be required. This, as a general rule, would probably be a safe one. But then it must be remembered that a variety of circumstances both as regards the disease and the patient, may render it necessary and important to deviate from this rule. In the first place there are many grades of congestion. In some cases there is simply an obstruction of some particular organ, followed by a considerable force of reaction or febrile excitement in the general system. Here, by abstracting blood, we take off that redundant excitement which is opposed to healthy organic function; secretion more readily takes place on the administration of suitable remedies, and the excitement becomes equalized. On the other hand, during the prevalence of congestive fever as an epidemic and at a late period of the season, the tone of the system may be prostrated so low that no reaction occurs. The pulse is weak, thready and rapid, the skin cold, and all the symptoms give evidence that the powers of the system are nearly overthrown. Here bleeding would be injurious, and would most probably lead to a fatal issue of the case. The reason why this should be the effect, is involved in no obscurity. The system generally and the infarcted organs particularly, would be thereby relaxed; and consequently, the congestion of the latter instead of being relieved, would be increased, and of course an additional quantity of blood would be thrown back upon the heart. This organ would be thus stimulated and its action increased, while the system, less able to sustain itself under this constant and additional excitement, would soon be exhausted and sink. In the winter of 1827, a friend of mine had a negro man, aged about thirty, and of a consumptive habit of body, who was seized with *hæmoptysis*—he had been repeatedly bled in the course of a few days, had taken calomel to act upon the biliary organs, and a solution of tartar emetic and nitre at short intervals. The disease advanced rapidly and threatened a fatal termination. The bleeding

aggravated all the symptoms; the cough and dyspnœa were increased, the discharge of blood more frequent and greater, the pulse small, weak and frequent, and the discharges from the bowels watery. I was called to see him, and advised a discontinuance of a course of depletion. A large blister was applied to the breast, a few grains of the sulphate of quina were administered, with a pill containing a small quantity of calomel three times a day. The tincture of digitalis was also given. It may be necessary here to remark, that the course of depletion in this case had been carried to such an extent as required the organic sensibility of the liver to be elevated before the calomel would be capable of exciting this organ to secretion—the quina had this effect; but this course in congestive fever would be improper—the suspension of the biliary secretions there, is the effect of *indirect* debility, and tonics, under such circumstances, could only aggravate the condition of the diseased organ. But to return to our case. In a few days the cough and hæmoptysis were gone, and the patient was entirely relieved. In this case there was a congestion of the liver with a deficient tone of the general system. Every bleeding increased this obstruction, and the base of the liver, greatly enlarged, pressed upon the diaphragm and prevented the due expansion of the lungs. The heart too, in consequence of the increased pressure of the circulation upon it from the hepatic obstruction, was unable to admit a free return of blood from the lungs, and their congestion was the result. This case, although not a case of congestive fever, will illustrate, in some degree, the impropriety of excessive bleeding in such a state of the system.

Secondly.—The age of a patient, with his general habits of body, should have some influence in determining upon the advantage or the hazard likely to result from bleeding. If the patient be young, of a sound constitution and a pretty full habit of body, the congestion must be extreme and the powers of the system greatly oppressed, if the abstraction of blood would be injurious. If a momentary prostration should ensue, the inherent powers of the system are so strong, especially if aided by moderate stimuli, that they will probably soon rally and

reaction will take place. And when this does occur, it will not be denied that the patient is in a more favorable state than he was before he was bled. The excitement afterwards is more easily equalized and sustained, and secretion from the oppressed organs is obtained with less difficulty.

Thirdly.—Another circumstance entitled to consideration on the subject of blood-letting in this disease, is the stage of the disease or the length of time which the patient has been sick. In the early stage of congestive fever, if no contravening circumstance forbid, it is almost an invariable practice with me to bleed. At this period some reaction may generally be discovered during the twenty-four hours, and as yet the powers of the system are but little impaired. But if the heart be permitted to beat violently but unavailingly upon the various obstructed organs, without the adoption of any means to prevent it, the excitement soon becomes exhausted and symptoms of debility make a rapid encroachment. But when the early stage has passed, and the general heat of the surface is below the healthy temperature, and great restlessness and delirium are present, when the organic functions are scarcely able to maintain even an imperfect and crippled existence, then bleeding would be fatal.

Fourthly.—There is yet another and an important consideration which should enter into an estimate of the propriety of bleeding in this fever. It is the previous treatment. If the energies of the system have been carefully preserved, if proper means have been employed to equalize the excitement and the circulation, by applying large sinapisms or blisters to the stomach and abdomen, then the prospect of doing an injury by bleeding is imaginary; but if timely and prudently resorted to, great benefit will probably be derived from it. On the contrary, if these measures have been neglected, if the resources of nature have been ignorantly and wantonly wasted by copious watery discharges from the bowels, if the oppressed organs have not been relieved by counter irritation, and if no support have been given to the declining powers of the system, by the administration of necessary stimulants, then bleeding will put

the seal to the fate of the patient and finish the work which the other treatment had begun.

These are some of the general principles which should control the judgment of the practitioner in regard to blood-letting. But then there are occasional peculiarities belonging to cases which deserve attention, and which may require some deviation from our general rules.

And first of these is old age. The great loss of power accompanying venous congestion, when superadded to the infirmities and decrepitude incident to age, offer an insurmountable objection to the use of the lancet in such a case. The pale and wrinkled face, the shrunken limb, the attenuated form, indicating the loss of vital energy and the great preponderance of venous plethora, sufficiently admonish us that the lamp of life needs replenishment and not depletion. The loss of blood then instead of a remedy would be a coinciding agent, not with the disease only, but with the downward course of nature.

Secondly.—Intemperance. The consequences of a debauched and dissipated life, are but old age in anticipation. The nervous system becomes enervated, the heart is incapable of carrying forward a strong and healthy circulation of blood, visceral accumulations take place, the chylopoietic organs lose their tone, and the whole frame languishes in debility and atrophy. The condition of the system is even worse than in old age. The infirmities of the latter are like the falling of the leaves in autumn, which only betoken that gentle and gradual caducity to which all nature is subject; whereas, the intemperate man resembles the tree whose roots have been devoured by the worm, and whose verdant and lively foilage has scarcely unfolded itself to the eye, ere it has withered and fallen to the ground. Such a patient, therefore, in a conflict with this disease requires all the remnant of constitutional power that he retains. But this alone will not be sufficient—the collapsing energies of the system must be braced and supported by a judicious administration of stimulants.

Thirdly.—The use of the lancet will probably be unnecessary where the patient labours at the time of his attack under any

previous disease, as scrofula, pulmonary disease, or chronic inflammation of abdominal viscera; because the tone of the system has been impaired by these affections, and consequently the power of reaction is proportionably diminished.

Fourthly.—The different degrees of constitutional vigor and the different employments of individuals may regulate to some extent the propriety of this evacuation. Those who have inherited robust and healthy constitutions from their progenitors, or whose avocations in life have been such as to lead to a full developement of the various corporeal powers, are more likely to be benefited by the loss of blood, than those of delicate constitutions, or whose employments require but little exercise of the body.

On Purgatives.—In congestive fever, in which the whole glandular system is oppressed and torpid, it becomes an important desideratum to excite secretion, in order to relieve the obstruction of these organs and equalize the excitement and circulation. Hence, the administration of suitable purgatives is an indispensable part of the plan of treatment. In order, however, that they may afford all the benefit of which they are susceptible, it is necessary that they should be administered with great prudence and discrimination. Without the exercise of much skill and prudence in the management of this class of remedies, they will not only fail in producing a salutary effect, but they will inevitably increase the evils which they were intended to remove. In this respect their operation bears a striking analogy to bleeding. If reaction, and a more uniform excitement do not succeed the loss of blood, the effect is bad; so if our purgatives fail to excite the biliary secretion, their operation tends only to increase the prostration of the system, and to render the want of equilibrium in the excitement greater and more difficult to restore. In the administration of purgatives, there are a few points important to be attended to, and in relation to which some practical remarks may not be superfluous.

First.—A proper selection of medicines of this class should be made. Some purgatives are better adapted to diseases of

an inflammatory diathesis, others to diseases of an opposite character. Some operate more particularly on the glandular system; others have no effect upon that, but excite a copious discharge from the serous exhalents of the bowels; some simply increase the peristaltic action of the bowels; others call into exercise the whole range of organic sympathies. The purgative medicines which are best adapted to the disease of which we are now treating, are such as excite the biliary organs to secretion, with as little expense as possible to the tone of the general system. These are calomel, spirits of turpentine, aloes, rhubarb, compound extract of colocynth, castor oil, &c.

Secondly. The proper administration of them. Under this head may be included, their adaptation to the states of the system, and their combinations. In almost all febrile diseases, some appearance of a stage of oppression and of a tendency towards reaction may be discovered every twenty-four hours. In some cases it is sufficiently distinct, in others so faint as to be noticed only by a careful and experienced observer. During the fore part of the day, therefore, which generally embraces the stage of oppression, it has been my practice to give calomel 10 grains every one or two hours according to the violence of the disease. Combined with this treatment, however, are employed stimulants internally and sinapisms or blisters to the surface. Of these I shall speak presently. Owing to the great degree of torpor which prevails, the calomel seldom occasions sufficient, if any evacuations from the bowels, and in the evening it becomes necessary to resort to other purgatives to effect this object. Pills composed of equal parts of calomel, aloes and rhubarb, three of which may be given every two or three hours, are well adapted to this purpose. If these should fail in procuring evacuations as will sometimes be the case where great torpor prevails, equal parts of castor oil and turpentine should be exhibited.

Thirdly. Important practical inferences are deducible from the appearance of the alvine discharges, and they should therefore be frequently noticed. They afford the most infallible criterion by which to judge of the condition of the patient, of

the necessity of changing our treatment, and of the prospect of recovery. Other symptoms may deceive us. The pulse, the tongue, the skin and the countenance are not always faithful advisers in this disease; but the information derived from the appearance of the alvine excretions may be safely relied upon. There are three different kinds of discharges accompanying biliary obstruction in this fever: first, thin discharges of a yellowish colour; secondly, thin and black or green discharges; and thirdly, a watery fluid, with a dark coffee-ground substance settling at the bottom: these discharges indicate different degrees of congestion in the order in which they stand; the last evincing an uncommon severity and obstinacy in the disease. Whenever any of these discharges are frequent and exhaust the strength of the patient, they should be restrained by the exhibition of ten or fifteen drops of tinct. opii. As soon as the dejections assume consistence, the symptoms are mitigated, and the danger of the patient's situation is greatly diminished; but it will be necessary to continue the use of our purgatives until there is a discharge of healthy bile, which will show that the obstruction of the liver is fully removed.

Fourthly. The doses of medicines should be proportioned to the tone, or state of irritability in the bowels. It has been, I apprehend, too much the practice among physicians to administer very large doses of purgative medicine in congestive fever; they give at first, rather a large dose under the idea that a disease of so much violence requires extraordinary measures for its removal; and not finding it to have the effect expected, they presume the dose was inadequate, and a second and much larger one is consequently given, but with the same or an aggravated unsuccessfulness. In the winter epidemic of 1815–16 to "one patient in twenty-four hours, Dr. Mann gave one hundred grains of calomel, one hundred grains of jalap, two ounces of castor oil and four ounces of the sulphate of soda, all of which produced only two small evacuations."* This practice and these quantities are by no means uncommon, but this instance is selected because it is easily referred to. Dr.

* Vide Medical Sketches, page 315.

M. ascribes the want of success from these medicines to the neglect of sufficient bleeding. In inflammatory diseases, bleeding, it is well known, promotes the operation of purgative medicine; but in congestive diseases, unless the abstraction of blood be followed by a revulsive action upon the surface, which cannot always be depended upon, the difficulty of procuring operations upon the bowels will be greatly enhanced. The principle that I wish to urge is that there must be a relative coaptation between the doses of medicines to be given, and the state of irritability in the bowels. In diseases of an inflammatory diathesis, there is an oppressed irritability, owing to the high tonic action that prevails; and it is therefore necessary to reduce the state of tonicity, before the irritability is relieved sufficiently to be acted upon by purgatives, and even then large doses will probably operate more satisfactorily than smaller—but in congestive diseases where this irritability, which is acted upon, and which in turn enables the bowels to act upon the medicine, is deficient, small doses occasionally repeated will operate more speedily and more effectually; or if the irritability be so far obtunded as to be insusceptible to the impression of purgatives, it must be exalted by means of stimulants. As an evidence of the truth of this position, I need only refer to a fact which is familiar to every physician of experience, viz. that castor oil in congestive diseases frequently passes through undigested and in its formal state; while such a thing in inflammatory diseases, is an unknown occurrence. Again, it has often been truly remarked that large doses of calomel do not operate upon the bowels as well as smaller doses; and the reason is that large doses transcend in their effect that state of irritability which has been termed the purgative point. I will mention another fact in further support of this view, which is all that the limits of this paper, already perhaps too much extended, will allow. A gentleman of my acquaintance and for many years a resident of this State, has often during the past season, when labouring under slight indisposition, or feeling approaching symptoms of fever, relieved himself immediately by taking simply a tea-spoonful of sulph. magnesiæ; which has at

different times always succeeded in bringing away black or dark green bilious discharges. Here this minute dose of epsom salts seems to have been precisely adapted to the excitability of the biliary organs and the bowels. Had there been a high degree of tone in the system, as in diseases of an inflammatory diathesis, this effect would not have taken place, because the quantum of stimulation would not have been equal to the morbid excitement. But this principle is not peculiar to purgative medicines; it belongs to every substance which is capable of making an impression upon the human body. Thus what are called stimulants are only so relatively; being stimulant in one quantity and sedative in another; stimulants when concurring with the state of irritability of the system, and sedative when the stimulus preponderates over the irritability. Electricity is a powerful stimulant when communicated to the system in a ratio correspondent to the powers of the system; but we all know it may become so accumulated as to extinguish irritability and destroy life. Where small doses of medicine, therefore, adapted as nearly as possible to the excitability of the bowels, are frequently repeated, and still the purgative effect is not obtained, it is useless to give larger doses or even to persist in the administration of smaller ones, until the excitability is roused by combining a stimulus with the purgative as oil and turpentine; or by equalizing the circulation and exalting the vitality of the system by means which I shall presently mention.

Stimulants.—We have already seen that a principal object in the treatment of congestive fever, is to equalize the excitement and to relieve the various organs which are oppressed by the internal accumulation of blood. This important object is to be effected by diverting the accumulation of blood from the internal organs to the surface, and by restoring the suspended secretions. I shall embrace under the general head of stimulants all that we have to say in relation to external excitants, as blisters, &c. as well as to the use of internal stimuli.

And first, external stimulant applications. These constitute no inconsiderable part of the means of relieving the system in this disease. They may be employed in various ways; as by

the hot bath, by frictions with turpentine, &c. and the application of blisters or sinapisms. The latter I prefer to either of the others. First—Because the impression they make is strong and powerful. The torpor of the skin is frequently so great, that the most pungent and intense stimuli are required to make a suitable impression upon it. In order, too, that they may afford a decisive benefit, their effect must be of a preponderant and engrossing nature; the surfaces, thus stimulated, must become points, to which the excitement of the whole system will radiate. When this object is realized, great relief is experienced, and an important step is taken towards throwing open the emunctories of the system and setting at liberty the imprisoned secretions—the nausea, the vomiting, the sense of fulness in the abdomen, and the præcordial oppression, the great thirst and sensation of internal heat, the anxiety that knows no intermission or abatement, the restlessness that finds repose in no change of posture, are greatly mitigated, and become merged in the severe but comparatively insignificant suffering from the inflamed surface. The system is now prepared for the administration of calomel and other purgatives, which before might have only concurred with the disease and increased perhaps the internal accumulation; and thus we should afford a pretext for the reproaches which are frequently cast upon us by the zealous followers of French pathology. Secondly—Sinapisms are to be preferred because their effects are more permanent. The impression made by a cantharides plaster is so feeble in cases of great torpor of the skin, as to make it doubtful whether vesication will be produced; and even when this does occur, the blisters are colourless, devoid of irritation and unaccompanied by any increase of sensibility in the part; they, of course, soon disappear without awakening the slumbering sympathies of the system, and without effecting any useful purpose. But the effect of sinapisms is powerful, and if they be permitted to remain until the skin be deeply inflamed, it will continue until the cause is removed for which they were applied. My practice has been to apply them early, and to a large extent of surface over the right hypochondriac and abdominal regions, es-

pecially in severe cases. But although sinapisms are greatly to be preferred as applications to the parts just mentioned, yet there are other parts where the torpor of the skin is not so great, and where blisters answer the purpose sufficiently well. When the brain is oppressed, as it almost always is in bad cases of congestive fever, a blister to the nape of the neck seldom fails of drawing well and having a good effect. By thus removing, in some degree, the burden from the sensorium, an increase of animation is imparted to the whole system.

With regard to the internal use of stimulants, the few observations which I have to make may be embraced under two heads. First—Diffusible stimuli. These consist of sulphuric æther, brandy, wine, &c. The first of these I occasionally used formerly, with benefit, in the last or typhoid stage of our common bilious fevers, but in congestive fever I have had no experience with it. With the two latter I have had a limited experience, but have not found them to produce any striking advantage, though I do not feel disposed to concur fully in the unqualified disapprobation which Dr. Daniell has passed upon them.* Secondly—The other stimulants are those which act generally and locally. Those which have some effect in elevating the tone of the general system, while they also have a peculiar effect in emulging the biliary organs. These are the capsicum annuum, spirits of turpentine and camphor. The difficulty of obtaining consistent bilious discharges by means of calomel and other purgatives, and the readiness with which they become converted into a thin and aqueous nature, has already been adverted to. This proceeds from a state of indirect debility in the penicilli of the liver by which their secretory action is suspended, requiring commonly the use of appropriate stimulants. In diseases where there is an ordinary degree of tone, calomel is sufficient to excite the liver to an increased action; and consequently to cause an augmented secretion of bile; but in diseases where the oppressed condition of the system is such as nearly to paralyze organic sensibility, ordinary excitants cease to have any effect. Hence arises a ne-

*Vide Daniell on Autumnal Fever, page 139.

cessity of combining them with stimulants which shall concur with and increase their efficacy. For this purpose the medicines which I last mentioned are well adapted. The red pepper I have employed in decoction, and in combination with calomel in pills. The decoction is administered in doses of half a wine-glassful every hour, and the powdered cayenne may be given to the extent of ten or fifteen grains two or three times a day. My plan, however, has been to give the calomel and cayenne combined in the fore part of the day, and pills of calomel, aloes and rhubarb in the evening, until sufficient evacuations are obtained. The decoction may be continued through the day, or suspended, as the condition of the patient may render expedient. Before I became informed of the beneficial effect of the capsicum, I was in the habit of using calomel and following it with equal parts of turpentine and castor oil. In some cases of visceral obstruction, this practice will answer every purpose; but in severe cases of congestive fever, the capsicum is decidedly preferable.

I may make a remark in this place which would have been introduced, perhaps with more propriety, under the head of purgatives; that if the discharges procured by the means I have just recommended, be watery, they should not be solicited; the exhibition of the purgative pills should be suspended, and a few drops of laudanum administered to check further evacuations of this character. When they are continued, the congestion must inevitably increase, the burden of the disease will as certainly become translated to the brain, and death must be the result.

Tonics.—In congestive fever there are different degrees in the extent of the congestion. Sometimes it seems to be principally confined to the liver and the other abdominal organs; at other times, the brain, the lungs, and the heart bear their full proportion of the disease. Each has its appropriate and diagnostic symptoms; but in all severe cases of this fever, the internal capillaries generally are engorged, and the symptoms arising from an oppressed condition of the brain are among the most marked and dangerous features of the disease. The

reciprocal influence of the brain and liver upon each other which has often been noticed in injuries of the head by many distinguished writers upon surgery, would go to prove that almost all affections of the liver are necessarily associated with a diseased condition of the brain, and vice versa; and certainly in all diseases in which either of these organs is deeply involved, this is no doubt the fact. But experience has sufficiently evinced the fact that the biliary organs may be obstructed to a certain extent without a correspondent congestion of the brain.

I shall consider the use of tonics, by which I allude particularly to bark or the sulphate of quina, in relation to each of these states of congestion.

First.—Where the congestion is principally confined to the abdominal organs. No matter what view is entertained of the proximate cause of this fever, it will be denied by no one, that when the disease becomes fully developed, the capillaries of the liver are in a state of congestion. The suppressed secretions prove it, the appropriate treatment confirms it, autopsic examinations demonstrate it. Here, then, unquestionably is a preternatural distention of the blood vessels of this organ; but as yet it is simply a remora of blood, and a torpid, inactive condition of the organ. A judicious administration of purgatives, combined with other suitable remedies, will relieve it and restore the patient to health. But if on the other hand, before this accumulation is removed by free and copious secretion, the sulphate of quina or bark be administered, the vital forces of the general system are augmented, the organic sensibility of the engorged vessels is increased, reaction ensues, and inflammation is established. These consequences are deducible from the most approved principles of physiology, and clinical results but too abundantly confirm their truth. Again; what are the symptoms of a patient to whom the bark has been thus prematurely administered? Does not the appetite fail, the spirits flag, and the mind grow uneasy and querulous? Do not the bowels become costive, the skin sallow, and the stools clay coloured, muddy or dark? Does not the pulse become frequent

or intermittent, the skin harsh and dry, and a feverish state of the whole system prevail? Do not the body and limbs become bloated and œdematous, and cough, hæmoptysis, and pulmonary consumption sometimes mingle in the melancholy train? Perhaps we are told, this disease is acknowledged to be a disease of debility, and that it consists simply in a disproportionate accumulation of blood in the internal organs; and hence tonics will give energy to the general system, and relieve the internal vessels of their engorgement. We may be told that the heart will thus acquire new power and the sluggish circulation will be thus propelled through the obstructed organs. To such we would reply, that the circulation in the capillary system is independent of the heart and arteries, and that, according to Bichat, "when the heart's action is increased, the exhalations are not, and the secretions are actually diminished." It is, therefore, impossible, upon anatomical principles, that the obstruction can be removed by tonics; and it follows conclusively, as the organic sensibility is thus increased and reaction of the general system is thus produced, local inflammation *must* be the consequence, if the powers of the system do not at once succumb to the general reaction. But inflammation of the liver, with its formidable consequences, is not the only evil to which the premature administration of tonics exposes the patient. Enlargements and inflammations of the spleen are common effects of this injudicious practice, especially in intermittents. These diseases, though not often attended with the same fatal and melancholy consequences as the disease just mentioned, are nevertheless productive of serious inconvenience and suffering, and frequently with an impaired state of the general health. Sometimes, also, when the patient has been subjected to this treatment, some other organ becomes affected by metastasis.

In the winter of 1826, I was called to see a man who was suddenly taken extremely ill. When I saw him his pulse was full, strong and slow, his face tumefied and red, and his whole frame was agitated with tremors. There was nearly a complete suspension of the intellectual faculties, and particularly of memory. He had forgotten his name, and supposed himself in Illi-

nois, from which place he had recently come. On questioning the family respecting his habits, and his previous health, I learned that he was a hard labouring man, but addicted to intemperance. About a week before, he had a chill and fever, for which he had taken nothing but the sulphate of quina; the disease returned in a day or two, and was again arrested in the same way: he felt better, and went into the swamp to cut wood, and while there he had got wet, and a diarrhœa had made its appearance; something had been taken to check the diarrhœa, and he had suddenly fallen into the state under which he was then labouring. This information afforded an explanation of his case, and the proper means of relief could not well be misunderstood. He was bled, a blister was applied to the back of the neck, and calomel was given and repeated until free secretions were obtained from the liver. The oppressed condition of the brain gradually gave way, and in a few days the man was able to go into the country. But few remarks are necessary upon this case. The dependence of function between the brain and the liver has already been adverted to. It is probable that the liver, each time the tonic was administered, was in a state of congestion. The man's previous habits of intemperance had predisposed the vessels of the brain readily to take on congestion, or in other words, the preternatural excitement to which these vessels had so long and so often been subjected, had impaired their tone, and the brain had become the weakest organ; and when the diarrhœa was checked by some stimulating astringent, the disease was immediately transferred to it.

Secondly.—In all severe cases of congestive fever, the brain, as has been before remarked, suffers from vascular engorgement; and it is in this variety, that the sulphate of quina displays its most pernicious, and frequently fatal effects. Nor is this condition of the brain an uncommon accompaniment of this disease. Let us adopt any view of the proximate cause of fever we please, let it consist in an impaired energy of the heart, producing venous congestion, let it be an engorgement of the internal capillaries, or let it be an inflammation of the

mucous membrane of the alimentary canal, the brain cannot remain long unassociated in the affection. Whenever congestive fever prevails as an epidemic, the brain is generally implicated in the disease. Among the first symptoms which present themselves in this fever are those which point to the brain as implicated in the disease. These are vertigo, confusion of the head, deafness, and dimness of vision, an intoxicated appearance of the eyes, &c. It would seem unnecessary, at the present day, to insist on the impropriety of the exhibition of tonics under such a condition of the brain; but as such a practice is still advocated by some, let us for a moment see how it will bear the test of analysis. The first effect of a tonic then, is to produce an exalted vitality in the stomach or the organ to which it is first applied; then, secondary action on the brain, produced by sympathy; and thirdly, subsequent disturbance of the heart, lungs, &c. Now, if there be already a congestion of the capillaries of the brain, a focus of irritation is established there; and as soon as the sulphate of quina is administered, the organic sensibility of the brain is increased, and with it inflammation must be the consequence. The tonic supplies the connecting link between congestion and inflammation. All that is wanted to convert congestion into inflammation, is an increase of vital power, and this deficiency is filled up by the tonic. Two kinds of inflammation present themselves, corresponding perhaps to the membranous and parenchymatous portions, as one or the other may be the seat of the disease. In the former, symptoms of phrenitis are developed, and the patient is seized with furious delirium. I have seen but one case of this kind, which occurred in a youth some years ago. In the other variety the symptoms are more obscure. In general there is headach, the tongue is dark and dry, the secretions are suppressed; sometimes there is delirium and bleeding from the nose, aural and visual deceptions, picking of the bed-clothes, &c. On the whole then, we must conclude that tonics cannot be either safely or advantageously relied upon in this fever. Physiology and pathology condemn them, analogy is opposed to them, experience is against them; and the time

is not distant, when the same righteous judgment which has been passed against them in typhus fever, will extend to the congestive fevers of southern latitudes.

December, 1830.

ART. II.—A *brief notice of Epidemic Pneumonia.* By Dr. N. SNEAD, of Washington county, Virginia.

THE object of the present communication is to solicit the attention of practitioners, particularly those who reside in mountainous and elevated districts, to a mode of conducting the abstraction of blood in those high grades of sthenic disease, which so frequently invade the country in the form of epidemics. The importance of exhibiting the peculiarities appertaining to epidemic affections, and that of instituting a successful practice, is notorious to the medical profession; as it is to diseases, in that shape, which refuse to yield to the ordinary resources, that therapeutics can date some of its most brilliant acquisitions. Climate in connexion with topographical and moral circumstances, combine to constitute agencies that are so predominant in their influence, on the human system, as to hold in subservience all our pathological inquiries. The relative force of the disease, its duration and its disposition to invade particular textures, are mainly attributable to the above causes: such is their potency in modifying our susceptibilities that epidemics which traverse districts of country remarkable for the diversities in question, are forced to assume corresponding discrepancies. In witnessing the march of some of our winter epidemics, the truth of the position is sustained and amply exemplified; for the affection that constitutes the subject of the following remarks has offered in its spread, a variety of pathological conditions which have produced the most conflicting views in regard to medical treatment. In the north-

ern parts of the union it has received the designation of pneumonia typhoides; in the western and southern states, pneumonia biliosa. In this part of the country pneumonic inflammation has not associated itself with typhus states of the system, but invariably exhibited the highest grade of sthenic action. Thoracic inflammation was introduced in this part of Virginia, in the winter of 1818. It was so formidable in its character, that few recovered who took it: every remedial process failed. Blood-letting that was so imperiously demanded, and which would have succeeded if conducted in the manner I shall point out, lost its reputation with many physicians; for those that were bled died as well as those who were not.

These unfortunate results necessarily gave rise to the most empirical and discrepant modes of treatment, which greatly increased its fatality. The same disease was epidemic, the two or three succeeding winters, in different parts of Virginia lying west of the Alleghany mountains, producing in its spread an unprecedented mortality. Pneumonic inflammation came on in November 1829, and continued until the succeeding June, few families escaping in a considerable extent of country. Its invasion was characterized by the most formidable array of symptoms: such as great difficulty of breathing, incapability of occupying a recumbent position, a short and suppressed cough without expectoration, a severe pain in the side radiating to different parts of the chest. These symptoms generally closed in death, if the most prompt and energetic treatment was not assiduously prosecuted on the 4th, 5th, or 6th day: though this terrific scourge was perfectly disarmed of all its terror by the abstraction of blood in such a manner as to maintain an approximation to syncope for the space of 30 or 40 minutes: as I clearly ascertained that venesection conducted after the usual plan, was totally incompetent to control it. And I have no hesitancy in saying, that the mortality which attended those epidemic forms of thoracic inflammation has been for the want of conducting the abstraction of blood in the manner I shall exhibit. When called to a patient labouring under a severe pneumonic inflammation, he was placed in a recumbent posture

and bled until symptoms approaching to syncope supervened; such as paleness, profuse perspiration, nausea, vomiting, a slow and almost imperceptible pulse: as the system partially recovered from the collapse, the flow of a few ounces of blood at short intervals, would maintain a relaxation of the system for the space of 30 or 40 minutes, which affected a complete alleviation of all the inflammatory symptoms. It is only known to those who have practised this mode of blood-letting, how small an amount permitted to flow at intervals, will maintain the system in a state of incipient syncope, after the first impression is made. The brief detail of a case or two, will suffice to present the striking superiority of this mode of blood-letting over that of erecting the patient, and bleeding to syncope without regard to protracting the impression.

Case 1.—Joe, an athletic negro man, was taken with inflammation of the lungs and serous textures of the chest, in March, 1830. Blood-letting was prosecuted in this case to the full extent, though after the usual manner. He complained of fixed pain in the left side, that did not radiate to any other point of the chest; his respiration was frequent and laborious, pulse full and strong. In 6 or 8 hours after the accession of the disease, he was erected and bled to syncope, which required the abstraction of three pounds of blood. The system in a few minutes recovered from the impression. Tartar emetic was given so as to produce some vomiting and keep up an incessant nausea. Calomel, with other purgatives, was also prescribed. On the second day, the inflammatory symptoms returned with their previous violence; the patient was erected and bled thirty-two ounces, which produced faintness; he was placed in a recumbent posture, and in a few seconds recovered from the relaxation and vomiting that had been effected by the blood-letting; tartar emetic, so as to nauseate, and mercurials were continued. On the third day the thoracic inflammation presented itself with the utmost violence. He was again placed upright, and two pounds of blood, drawn from a large orifice, produced syncope, from which he immediately recovered by being placed horizontally. This abstraction of blood effected a striking

alleviation of the symptoms. A large blister was applied at this stage. On the fifth day a pain in the side and great difficulty of breathing returned. He was erected and bled 20 ozs. which produced extreme sickness, profuse perspiration and a failure of the pulse; a condition from which he soon recovered. During the whole progress of the disease, he had a slight cough without expectoration. Extensive blistering, mercurials, antimonials and purgatives were continued. Notwithstanding the copious sanguine depletion, the thoracic symptoms were not perfectly removed; for a full inspiration would excite a convulsive cough with pain in the side. On the 6th day the inflammation returned with redoubled violence; his pulse was full and frequent, severe pain in the side, difficult respiration, incessant moan and hot skin. As the symptoms were threatening he was erected and bled one pound and a half, which produced complete syncope. The buffy coat was half an inch thick. Tartar emetic was exhibited in 3 gr. doses every two hours with calomel and nitre, under the apprehension that the further abstraction of blood could not be tolerated, the tartar emetic affording the only resource in arresting the inflammation. The next morning respiration was nearly natural—extremities cool—was disposed to sleep—could make a deep inspiration without exciting pain—the pulse was greatly reduced in frequency, though full and strong, which represented the hopeless condition of the patient, as effusion was manifest. He remained in this state from Friday until Sunday 12 o'clock, at which time he expired. A post-mortem examination presented the following morbid appearances: the left lobe of the left lung exhibited manifestations of the highest grade of inflammation, it being of a dark red colour. The ramifications of the bronchiæ, when divided by transverse incisions, were filled with a purulent fluid. The inflammation was entirely limited to the left lobe of the left lung, no other portion of the lungs exhibiting recent marks of disease. The inflamed lobe was attached to the pleura costalis by a thick layer of coagulable lymph that separated by the slightest touch. The pleura costalis was highly inflamed a small space around the adhesion of the lobe.

The pericardium contained about an ounce of serum, though it presented no vestige of inflammation. The abdominal organs were sound.

This case of pneumonic inflammation, which has been succinctly noticed, we perceive resisted the united energies of frequent blood-letting to syncope, tartar emetic, mercurials, blisters and purgatives. Hundreds have fallen a sacrifice to uncontrolled thoracic inflammation, in the south-western parts of Virginia, by pursuing the course of practice that has been detailed in the above case. The patients were erected and bled to faintness without regard to the quantity of blood drawn, or the maintenance of a prolonged relaxation of the system; the physician believing every thing atchieved of which the lancet is capable. The sporadic cases would generally yield to this plan; though the disease when epidemic bid defiance to this mode of blood-letting.

It was by recognizing the total incompetency of the ordinary mode of sanguine depletion, that I was induced, in the epidemic under consideration, to resort to venesection in a recumbent position, and push the lancet to an approaching syncope, and also maintain that condition 30 or 40 minutes, or as long as the exigency seemed to require. The following case will furnish an example of the mode, in which I have drawn blood, in more than one hundred and fifty cases of thoracic inflammation, with invariable success.

Case 2nd. James Lyon, of sanguineous temperament, in April 1830, was seized with inflammation of the lungs and pleura: he was bled 24 ozs. in a few hours after the developement of the disease by a bleeder in the neighbourhood: this amount produced syncope, as it was drawn in an erect posture. The second day, he was bled 12 or 15 ozs. whilst upright, which produced deliquium. I saw him on the third day, for the first time. He had been puked and purged freely; all the manifestations of a vehement pneumonic inflammation were rapidly progressing; he was bled, whilst lying, 32 ozs. this produced sickness, paleness, profuse sweating and slow respiration. In a short time, he partially recovered from the impression though

by permitting a few ounces of blood to flow, at short intervals, an incipient syncope was continued for the space of 30 or 40 minutes: this effect upon the system produced a perfect alleviation of the pain and difficult breathing. Antimonials, mercurials, and purgatives were prescribed. On the 4th day severe inflammation returned. While recumbent he was bled 30 ozs. which produced an approach to syncope; as the heart and arteries imperfectly resumed their energies the flow of a few ounces of blood kept up a complete relaxation with nausea for half an hour. Antimonials, calomel and purgatives continued. On the 5th day, difficult breathing, severe pain in the side spreading over the chest returned: the loss of 16 ozs. of blood drawn when recumbent, produced great sickness with general relaxation, which was continued 20 or 30 minutes, by the subsequent flow of a few ounces of blood. A large blister was applied at this stage: such was the rebellious character of the affection that inflammation returned on the 6th day, though with less violence than on the preceding days: the abstraction of 16 ozs. of blood, in the mode exhibited above, removed every vestige of inflammation, which was followed by a speedy convalescence. Lyon lost upwards of 9lbs. of blood in 6 days; but if the same amount had been drawn in such a manner as not to have maintained a prolonged relaxation of the system, at each abstraction, he would have died from the results of unrestrained inflammation.

The peculiar obstinacy that characterized this case was owing to the too limited depletion, on the first and second days. A large majority of patients had to be bled recumbent, as not half the requisite amount of blood could be abstracted, if it were drawn in the erect posture. In those comparatively few cases that could sustain the loss of 40 or 50 ozs. before symptoms of fainting supervened, they were bled in the upright posture; though when placed horizontally small quantities were permitted to flow at short intervals so as to keep up a relaxation for half an hour. This prolonged prostration of the heart and arteries, not only gives the capillaries time to free themselves of the congestion that constitutes the seat of inflamma-

tion, but effects striking modifications upon the organic sensibility presiding over other portions of that system, as there are called into action important revulsive processes, which are simultaneous in their developement, and are sustained so long as the collapse is continued. The mucous lining of the intestinal canal, the skin, the liver, and kidneys are made to pour forth their respective secretions in profusion.

Since I have recognized the superlative advantages of maintaining a protracted relaxation of the system in thoracic inflammation, I have extended the practice to other forms of phlegmasiæ, as well as to highly concentrated shapes of fever, with the most happy results, as I have thereby greatly abridged their usual periods of duration, and rendered their progress comparatively lenient. In those sporadic forms of phlegmasiæ and fevers, an incipient syncope was always produced in an erect posture; the patient afterwards being placed horizontally, and the relaxation maintained, for the space of 20 or 30 minutes by the subsequent flow of a few ounces of blood. All cases requiring blood-letting to deliquium, can sustain an approach to syncope, for the space of 20 or 30 minutes without hazard of producing a dangerous collapse. In the abstraction of blood in the manner noticed I have not witnessed, in a single instance, a dangerous collapse, which sometimes follows an inordinate blood-letting; nor an increased irritability, followed by violent reaction of the heart and arteries: a condition that occasionally supervenes upon excessive sanguineous evacuations. This mode of bleeding frequently renders the repetition of the lancet unnecessary; as it at once arrests the progress of inflammation in a great many cases. In one case of severe pneumonic inflammation, four pounds and a half of blood were drawn at one time, in the erect posture before syncope was produced; it was succeeded by a relaxation that continued near an hour, during which time a general secretory process was going on, and the patient recovered without the repetition of the lancet.

In this high grade of pneumonic inflammation, there was not the slightest bronchial secretion in the first stage, nor did ex-

pectoration accompany the second stage, if evacuations of blood were profuse. Physicians are disposed to attach too much importance to a copious secretion from the lungs in peripneumonia. It is regarded as a means of procuring a safe and early resolution of the disease. I have in many instances seen the premature suspension of the lancet from the circumsance of the supervention of a free expectoration, when its proper employment would have prevented it, or even removed it. Why should we depend upon a bronchial secretion to carry off a partially subdued inflammation of the lungs, any more than trust an imperfectly reduced phlegmasia of other mucous membranes to an increased secretion? We are thus surely exposing to imminent danger the most important organs of the economy by an unnecessary protraction of the inflammation. How frequently do we see the developement of tuberculous affections the result of such delays, which the judicious use of the lancet would have prevented?

There is an insidious form of pneumonic inflammation, that we frequently meet with in practice, which, without the utmost vigilance, is permitted to make dangerous advances before sufficient blood-letting is resorted to: the symptoms are, a soft and frequent pulse, vanishing upon the slightest pressure, inconsiderable pain, a free perspiration of the trunk and superior extremities; the patient lies upon his back and complains but little; has a short cough without expectoration; the breathing is frequent and short; the thorax being but very imperfectly expanded, respiration is principally executed by the diaphragm and abdominal muscles; and it is to the manner in which this function is performed that we are to look for the extent of danger as it represents the real condition of the thoracic organs. The serous textures in the first stage are not implicated, the congestion being confined to the parenchyma of the lungs: copious blood-letting renders the pulse slower and stronger, and thus unmasks the case. Patients in this situation cannot sustain blood-letting in an erect posture, as the loss of a few ounces produces syncope—though when recumbent, copious bleeding can be tolerated, not only with safety, but advantage.

ART. III.—*Thoughts on Epizootic Worms, and their effects in producing disease, with practical remarks.* By CHARLES CALDWELL, M. D.

IN preparing this article, it is not my intention to write an essay on epizootic worms in general. The task would be too weighty for my present leisure, and would lead to an extent of inquiry and composition, far beyond what I contemplate. My remarks shall be restricted to those worms that are found in the human body. Nor will they include the whole of them; but only such of them as I have myself seen and examined; and into whose character, and effects on health I have personally inquired.

The worms which I purpose to notice, inhabit the alimentary canal, and are of two kinds, the Round and the Flat. The round kind includes three species, the *Ascaris lumbricoides*, the *Ascaris vermicularis*, and the *Trichuris*. Of these the first and third species are found chiefly in the small intestines, and the second in the rectum, especially the lower portion of it.

Of the Flat kind there are two species, the *Tænia vulgaris*, and the *Tænia lata*. The Cucurbitini, so called from their resemblance to the seeds of the gourd, were once believed to be a third species. They are now known, however, to be nothing but joints or fragments of the *T. vulgaris*.

The Tænia cannot be said to inhabit any given portion of the intestines, as, from its great length, it sometimes extends throughout the whole canal. Presuming that most medical readers, not excepting even those who are but students of the profession, have often seen intestinal worms, and are familiar with their appearance, I shall not describe them.

To those who are devoted to philosophical research, the origin of these parasitical beings is a curious and interesting subject of inquiry. Except in the alimentary canal, and sometimes in the liver, muscles, and other deep-seated organs of man and other animals, they are nowhere to be found. Exterior

nature does not produce them. From without, therefore, they cannot be received. With neither our food nor our drink are the animals or their eggs swallowed, because they exist in neither of them. From whence, then, are they derived? Do they belong to us as a natural and necessary accompaniment, imparted to us by parental influence? Are they the product of diseased secretory action in the alimentary canal and liver? Or, are they generated there, by any other more secret and equivocal mode of action.

As it is my intention to confine myself to facts and settled principles, I shall not enter on the discussion of the metaphysicks of these questions. How far parental influence may extend, physiology, as yet, is incompetent to inform us. Pathology sheds no definitive light on what may be the entire product of morbid secretory action. I mean that such action may produce results much more extraordinary than we are prepared to anticipate. I believe it does occasionally give rise to effects which but few have even imagined; for its power is great, and its range extensive. In its modifications and its issue secretion is much more multifarious than any other process of living matter. Nor have we any positive knowledge as to the power of Nature to form, consistently with her existing laws, one living organized being, without the instrumentality of another.

Amidst the difficulty and obscurity, however, that surround the subject, there are reasons which notwithstanding compel us to believe, that by some specific processes of its own, the human body does form the various worms that inhabit it. I have used the phrase "specific processes," because each species of worms being essentially different from every other, must be produced by a kind of action peculiar to itself. When the matter is strictly inquired into, there appears to be no other origin to which epizootic worms can be ascribed. Both the tænia and the ascaris lumbricoides, and, I believe, the trichuris also, have been found in the alimentary canal of the fœtus in utero. The tænia moreover has been often detected in the livers, muscles, and other inaccessible parts of various subjects of the animal kingdom. Hydatids are also daily discovered in similar situations.

Nor is this all. Of worms that are non-descripts, the same is true. About the close of the revolutionary war, an animal of this sort was found in the liver of Mrs. H., a lady of some note, who died in Philadelphia of an unknown disease. The worm was so large as to occupy a great portion of the viscus that contained it, and was of a species unknown to every naturalist who examined it. As far as I am informed, its like has never been seen either before or since. The dissection was witnessed by the late Professor Rush who spoke of it in his lectures; and the fact is recorded in the "American Museum," the only monthly or quarterly periodical, published at the time, in the United States. In medical writings other similar cases are reported.

For the production of these worms, I repeat, there seems to be but one source, which reason can approve; and that is, specific processes of secretory action in the animals containing them. Even conjecture is at a loss for any other mode to account for the existence of a worm in the alimentary canal of the fœtus in utero, or in the liver, ovarium, or any other internal and deep-seated organ of a living being. Nor is the supposition, that it may be the product of morbid secretion, either extravagant in itself, or inconsistent with other well known phenomena. On the contrary, appearances in corroboration of it are numerous and strong. Tumors possessing life, and as dissimilar in structure to the parts around them, as a worm is to the mucous lining of the alimentary canal, are often found in the human body. Yet by the secretory action of those parts, such tumors are necessarily formed. Of this, cancer may be adduced as a suitable illustration. So totally dissimilar to the parts around it is that tumor, that it is considered, by many, a parasitic animal. If cancer, then, can be secreted from the blood, why not also an epizootic worm? To this question, my opponents, should I have any, are bound to give an answer. I myself cannot. I must observe, however, that these worms, when once produced, propagate afterwards like other animals of their class. In alleging, therefore, that they are the result of morbid secretory action, in the animal

that contains them, I allude to their primitive production in that animal. No solid reason can be rendered why beings produced in this way may not be prolific, as well as those of parental offspring.*

Are worms always and necessarily a cause of disease? They are not. They *excite* disease only at times, and in conditions of the system favourable to it; those conditions having been induced by other causes. I have intentionally said that they "excite disease," for they act only as the exciting cause. As the remote cause, inducing predisposition, they act very rarely, if at all.

Worms, especially the three species of round ones, are found chiefly, but not exclusively in the bowels of children. Those of adults are also inhabited by them. But they abound most in the intestines of valetudinary adults. In those who enjoy vigorous health, their number is smaller. As relates to children the same is true. In the intestines of all of them worms are found. But they are most numerous in the intestines of those whose health is imperfect. And it is in such alone that they prove an exciting cause of disease. Or if they ever prove so in others the event is rare. When they do act deleteriously, what are the diseases which worms excite? They

*It is known to many that I began, at a very early period of my medical life, to contend for what is called the "equivocal generation" of epizootic worms; I mean their production by abnormal secretion. At that time the doctrine was pronounced visionary, and was very unpopular. As far as I was informed, it had not, when I first embraced it, a single advocate in the United States. Nor, indeed, did I know of its having one any where. It was even denounced by some writers as unfriendly to religion. But the views of physicians respecting it are rapidly changing. It has now many advocates, and, with the increase of physiological knowledge, is destined to have many more. It will become universal, because it is true.

As relates to generation, the maxim "omnia ab ovo" is now changed to "omnia a vivo." Neither is true. Another step in liberal science is yet to be made. The maxim on which physiologists will be compelled ultimately to rest will be "omnia viva a vi naturæ." Nature has the power to produce living beings, without the instrumentality of specific parentage. The formation of *animalculæ infusoriæ* is proof of this. Nor does an adherence to this opinion involve a belief that a mere "fortuitous concourse of atoms" can form a being possessed of life. All is the result of settled laws, as might be easily made appear, were this a suitable occasion to discuss the subject.

are various. Worms may give rise to most complaints, that result from gastric and intestinal irritation. They excite epilepsy, tetanus, hysteria, and other convulsive affections. They produce also pulmonary disease, obstinate cutaneous eruptions, hydrocephalus internus, and even madness. But they attract most attention, as the reputed cause of what is denominated "worm fever." To their operation, therefore, in the production of that complaint, shall my remarks be chiefly confined in the present article. As relates to the other specified forms of complaint, I shall here only further observe, that the same verminous irritation, which in one individual excites fever, will, in others, excite those other forms, according to the existing predisposition of the system to disease. For the predisposition always determines the form of the malady.

I have said that it is only in consequence of the existence of a certain condition of the system, that intestinal worms excite fever. This position rests on a twofold ground. The worms are constantly present in the intestines of children, while the fever occurs only occasionally; and when it does occur, it is often removed, without any discharge of worms. To these two reasons might be added a third. The mere discharge of worms will not remove the fever, unless it be accompanied by proper secretion. But this secretion, when it occurs, cures the fever, without the evacuation of a single worm. On this topic I shall be more particular hereafter.

Am I asked, what is the condition of the human system in which worms become productive of fever? I answer, a morbid irritability of the intestinal canal, accompanied by congestion in the part, and extending perhaps in some degree to all the abdominal viscera. This congestion existing, and creating a predisposition to fever, worms act as the exciting or directly productive cause, by means of irritation. And were no worms present, the predisposition existing, an exciting cause would not be long wanting. Some other irritating substance would act as such. Any irregularity in diet, relating either to quantity or quality, would be sufficient for the purpose. A meal superabundant in amount, or of an indigestible charac-

ter, is not only a frequent, but a very efficient exciting cause of fever. Nor is this all. Let the irritability of the alimentary canal be very high, and the predisposition to fever strong in proportion, and a common meal, or one even lighter and more digestible than common, will then prove the exciting cause, and produce the disease. This is an event which every physician of experience in his profession, must have often witnessed. There is no other ground, on which many attacks of disease are explicable. But this is consistent with the principles of pathology, and, when, fairly understood, appears satisfactory.

Am I again asked, may not worms themselves first give rise to the congestion and predisposition, and afterwards act as the exciting cause, and produce the fever?—If they be exceedingly numerous, they may *possibly contribute* to the original formation of congestion, and then irritate, and excite fever. But such an occurrence, I repeat, must be rare. The congestion is almost universally the effect of other causes, to be mentioned presently, while the worms do nothing but subsequently irritate, and augment it.

An opinion prevails in the community, that worm-fever occurs most frequently at certain lunar periods; say the full or change of the moon; or at both times, attacking twice during a lunation. Nor, although this is the belief more especially of the unprofessional portion of the public, is it confined entirely to that. Not a few physicians, some of them eminent, are its advocates. Is the opinion worthy of credit? I apprehend it is; and I found my belief on the following considerations.

If I am not mistaken, the opinion comports alike with observation and reason. I have myself attended children, who were, with no little regularity, attacked by fever considered verminous, about every full or change of the moon. To other physicians similar cases have often presented themselves. And there are not wanting weighty causes why it should be so. For the further elucidation of this subject, a brief consideration of lunar, or sol-lunar influence is requisite.

That the moon acts powerfully on the waters of the ocean,

is an axiom in astronomy. And this influence is much greater at the times of her full and change, than at any other period. But it is certain that on the water she cannot thus act, and raise tides, without acting, at the same time, on the atmosphere, which lies between her and the water. Indeed it must be in some measure by acting on the atmosphere, and diminishing its pressure, that she produces the tides. Lighten that pressure on a given surface of water, while it continues undiminished all around it, and the water will rise in the partial vacuum thus formed, precisely as it rises in a common pump. But the belief that a strong lunar influence is exercised on the atmosphere does not rest alone on an inference from premises, however sound may be the one, and however fair the other. It is sustained by observation in all times and countries, whose history is known to us. And every one who is willing to employ his senses on the subject, free from bias, may verify it by his own observation.

Of all the violent commotions that take place in the atmosphere, in the shape of storms, tempests, tornados, and inordinate falls of rain, hail, and snow, a very great majority occurs about the full and change of the moon. Of great and sudden vicissitudes in temperature, especially from heat to cold, as well as of striking changes from dryness to humidity, the same is true. They also follow the changes of the moon. Observation to this effect is as old as the time of Aristotle, in whose writings the fact is recorded. Were it requisite I could show, by the most authentic documents we possess on the subject, that even earthquakes, and the eruptions of volcanoes, are subject to the influence of the same planet. The most violent of them, with a majority of those that are less so, appear about the periods of her full and change, or of her apogee or perigee. So powerful is her influence, not merely on the surface, but the deeper-seated parts of our globe. Those who are desirous of detailed information on this subject, are referred to the first volume of Webster's "History of Epidemic and Pestilential Diseases," a work with which every physician should be familiar.

Nor is the influence of the moon on the earth, confined to dead matter. If reliance can be placed on human observation and veracity, it extends to both vegetables and animals. Many experienced agriculturists assert, that, all other things being alike, seeds vegetate most rapidly and favorably, when they are sown during the light of the moon. Nor are principles wanting to sustain a belief to this effect. Light is not only useful, but necessary, to vegetation. In utter and continued darkness, plants would not grow; or, if at all, they would grow very feebly. But the solar light, reflected from the moon, is better than the entire absence of light. Vegetable seeds, therefore, planted during moon-light, receive the influence of a greater amount of that agent, than those that are planted during the dark of the moon. Hence they vegetate with more rapidity and vigor. There is ground, moreover, to believe that the moon acts on the vegetable kingdom, through other channels than her light. In constructing vessels, which they wish to be durable, ship-builders give a preference to timber that has been cut down during the dark of the moon. They find it less perishable than that which is cut during moon-light. With them, this is the result, not of theory, but experience, the readiest and safest guide to truth.

As relates to the animal kingdom, the same principles seem to prevail. The influence of the moon on shell-fish is proverbial. To the epicures of ancient times, those of luxurious Rome in particular, it was well known, that these animals are a more juicy and delicious article of fare, during the light, than they are during the dark of the moon. And, on this point, modern epicures fully concur with their ancient brethren. Nor is this the entire extent of lunar influence on animated nature. The substance of every animal, whether, fish, fowl, or quadruped, killed during the light of the moon, putrefies sooner, all other circumstances being equal, than it does when killed during the period of her darkness. Such, at least, is the declaration of those, whose experience seems sufficient to disclose the truth to them, and who have no theory to maintain on the subject. Expose, as they tell us, two similar masses of dead

animal matter, one to the external atmosphere during a moonlight night, and the other to the atmosphere during a dark night, the temperature and humidity being, in each case, the same, and the former will make the greatest progress in putrefaction.

It has been already intimated, that her light is not the only medium through which the moon extends her influence to the earth. Either alone, or in union with the sun, she produces in the atmosphere, by means of attraction, as regular and uniform changes, which may be denominated tides, as she does in the ocean. As far as I am informed, in relation to it, this phenomenon was first discovered, many years ago, by a Mr. Howard, keeper of the Observatory of the Royal Society of London. But his report on the subject, which was published in Tilloch's Magazine, never attracted much notice. For our knowledge respecting it, we are indebted chiefly to Dr. Balfour, Physician General to the British forces in India. His efforts to throw light on this topic, were unparallelled in labour, and successful in their result. Throughout one entire lunation, a period of twenty-eight days and a few hours, he examined his barometer, *in person*, every thirty minutes. The issue was, a clear and satisfactory ascertainment of the existence of the atmospherical tides, of which I have spoken. These commotions in the atmosphere not following precisely the movements, or corresponding with the positions of the moon, the Doctor attibuted them to what he denominated "sol-lunar influence," which he supposed to result from the united action of the moon and sun: and whose powers as relates to the medical profession, he found to be equally interesting and important. To express his views on the subject, in his own language, he discovered, in the course of the experiments and observations, to which I have referred. and which were made in Calcutta.

"1. That in the interval between ten at night, and six in the morning" (eight hours) "there existed a prevailing tendency in the mercury" (of the barometer) "to fall."

During this period the pressure of the atmosphere *downward* was diminished; whence it is plain, that, by sol-lunar influence

or attraction, the matter of the atmosphere was elevated, in the form of a tide, which is tantamount to the abstraction of a portion of its gravity.

"2. That in the interval between six and ten in the morning" (four hours) "there existed a prevailing tendency in the mercury to rise."

During this period, sol-lunar influence or attraction was weaker, and hence the pressure of the atmosphere was greater. The airy ocean that surrounds our globe, was now in a state of ebb; whereas, in the previous case, it had been in that of flood.

"3. That in the interval between ten in the morning and six in the evening" (eight hours) "there existed a prevailing tendency in the mercury to fall."

The amount and action of sol-lunar influence now, were the same as they had been in the interval between ten, at night, and six in the morning.

"4. That in the interval between six and ten in the evening" (four hours) "there existed a prevailing tendency in the mercury to rise."

During this space the relation between the atmosphere and the heavenly bodies was identical with that which had existed in the interval between six and ten in the morning.

Not willing to confide in his own experiments and observations alone, Dr. Balfour induced a large number of physicians and surgeons, not less than from fifty to sixty, in various parts of the British oriental provinces, to engage in similar courses, which terminated uniformly in similar results. By each individual engaged, the existence of regular tides in the atmosphere was perceived. The fact, therefore, may be considered established. Hence it appears, that, during every natural day, there occur in the atmosphere, four stated changes, besides a number of intercurrent and irregular ones. During sixteen hours of the time, its pressure is comparatively light, and heavier during the remaining eight.

Somewhat corroborative of this view of things, are the sentiments of all those who are observant of the weather, more

especially of such as follow the sea. They know that gales and tempests usually begin and terminate at given hours. These hours are the intervals between six and ten in the evening and morning, and between twelve and two in the morning, and twelve and two in the afternoon. During these periods, which amount to twelve hours of the twenty-four, a greater number of gales and tempests, in the proportion of five to one (perhaps I might even double the proportion) begin and terminate, or at least abate, than during the remaining twelve hours of the natural day. Hence, to those hours, when storms either threaten, or are raging on the ocean, mariners look with great solicitude. And every common observer must be sensible how frequently a boisterous day becomes calm in the evening, and a boisterous night toward morning. During the summer season, moreover, thunder-clouds usually make their appearance between twelve and two in the afternoon.

But Dr. Balfour and his associates did not limit their observations to the mere *existence* of atmospherical tides, but extended them to their *effects*. And they found disease to be greatly influenced by them. They discovered, in particular, a striking coincidence, in point of time, between the ebbing and flowing of those tides, and the access and recess of febrile paroxysms. Among those who inhabit the sea-coast, a conviction prevails, that between the fatal termination of diseases, and the movements of the tide, there exists, in point of time, a manifest connexion. Hence the common prediction, respecting persons who are considered in a moribund state, that they will die "about the turn of the tide."

But other authors, besides Dr. Balfour, have written ably on lunar influence in the production of disease. Among these Mead and Moseley are conspicuous. The facts which they adduce in support of their opinions are highly interesting, and throw much light on the subject which they concern. For full and very instructive details, the reader is referred to Mead's "Treatise on solar and lunar influence," and Moseley's "Treatise on tropical diseases." The result of their observations is, that the following complaints are obviously under the

influence of the moon; a sentiment in which an abundance of facts which I have myself witnessed compels me to concur.

1. *Hæmorrhages.* Unless when brought on by accidental violence, almost every severe and extensive hæmorrhage occurs about the full or change of the moon. This is perhaps more especially true as respects hæmoptysis. In the course of many years' observation, I have never witnessed a very copious or fatal spontaneous hæmoptysis, that did not take place about one or the other of the periods designated.

On this topic Dr. Mead gives the following very singular narrative.

"Dr. Pitcairn's own case is remarkable, both in regard to the disease (hæmorrhagy) and its concomitant circumstances. In the year 1687, being at a country seat near Edinburgh, in February, on a fairer day than usual at that season, and the sun looking reddish, he was seized, at nine in the morning, the very hour of the new moon, with a sudden bleeding at the nose, after an uncommon faintness. And the next day, on his return to town, he found that the barometer was lower at that very hour, than either he or his friend Dr. Gregory, who kept the journal of the weather, had ever observed it; and that another of his friends, Dr. Cockburn, professor of philosophy, had died suddenly, at the same hour, by an eruption of blood from his lungs; and also that five or six others of his patients were seized with different hæmorrhages." This is analogous to the well known fact, that hæmoptysis and other forms of hæmorrhage often attack those, who ascend to the rarified atmosphere of very lofty mountains. In such places the mercury in the barometer always low.

Dr. Moseley's narrative of the case of Captain Richard Boyle, of the British army, is still more remarkable. The complaint was hæmoptysis, which in the interval between the 14th of February and the 9th of August, 1786, attacked the subject of it seven times, each attack being about the time of the full or change of the moon. Were it requisite, a great number of other cases, similar in character, might be easily adduced.

Nephritic complaints, especially calculous ones, usually at-

tack at lunar periods, particularly about the full or change of the moon. In proof of this, very many striking facts may be derived from the works of Mead and other writers, as well as from the experience of living physicians.

Of Asthma the same is known to be true. Sir John Floyer, who was subject to that complaint for more than half a century, has left us his testimony to this effect. From his own account, the paroxysms returned about "once a fortnight, and usually near to the full or change of the moon." In favour of this opinion medical writings contain an abundance of testimony; and the daily observations of many physicians concur in support of it.

Epidemics. To every one who has been strictly observant of their progress, it is known that epidemical complaints attack most frequently, as well as most severely, about the times of the full and change of the moon. Speaking of the plague, which prevailed at Nimeguen, in 1636, Diemerbroeck observes, that "the contagion constantly increased about the full and change of the moon; and that to the greater part of those who sickened at these times, the complaint proved fatal." Ambrose Pare asserts, as the result of much observation on the subject, that "people are most subject to the plague, at the time of the full moon." Mosely tells us, that in April 1780, eighty-seven men out of two hundred, on board of the Hinchinbrooke sloop of war, lying in San-Juan river, were attacked in the night, in the space of a few hours, at the time of the full moon, by a prevailing malignant fever, of which nearly all of them died. Statements of a similar character are contained in the works of Joubertus, Fincelius, Morellus, Heurneus, and other distinguished medical writers. From personal observation, as well as the concurring report of others, I know that yellow fever attacks most frequently, and proves most frequently fatal, about the periods of the full and change of the moon. To this the records of the disease in Philadelphia, New York, and Baltimore bear witness; and the same is true of influenza and measles, when prevailing epidemically.

Physicians of all ages and countries have remarked, that

sick persons, in a condition of great weakness, are injured by eclipses of the sun and moon, which occur at the times of the full and change of the latter luminary. Thus says Ramazzini, in his account of the purple fever, which prevailed at Modena, in 1693, "What happened on the 21st of January, was very surprising. For the moon having been eclipsed that night, the greatest part of the sick died about the very hour of the eclipse; and some were even struck with sudden death." Ballonius has recorded a similar case. On account of some peculiarity in his constitution, Lord Bacon uniformly fainted on the occurrence of a lunar eclipse, although he was not thinking of the event, and did not even know that it was about to take place.

As relates to the influence of the moon, on other diseases, Moseley has given us much interesting information.

"Apoplexies and palsies, says he, are very common among aged people; and in the course of fifteen years, at Chelsea Hospital, I have rarely known an instance of either of these disorders happening at any other time than at the change, or at the full of the moon." The Doctor further states, that "almost all people in extreme age, are attacked with their death illness, or die at the new, or at the full moon." In confirmation of this he observes, "of twenty-four instances of the death of men between 86 and 100, which have occurred in Chelsea Hospital, within these few years, thirteen died at the new moon, and seven at the full moon. Of two hundred and one persons, upwards of 70 who, within twelve years, had died in a charitable institution erected in London for the reception of aged invalids, he found that seventy-four had died at the full moon, and sixty-one at the new moon. Dr. Monsey, predecessor of Dr. Moseley, in Chelsea Hospital, died in London, at the age of 96, in December 1788, a few hours before the new moon. These views are further supported by the periods of the deaths of some of the oldest persons of modern times.

Thomas Parr died on the 15th of November, 1635, aged 152, at the time of the full moon.

Henry Jenkins died on the 2nd of December, 1670, aged 169, at the time of the new moon.

Elizabeth Steward died in London, on the 1st of May, 1725, aged 124, on the day of the new moon.

William Leland died on the 7th of December, 1732, aged 140, on the day after the new moon.

John Effingham, of Cornwall, died on the 6th of February, 1757, aged 144, near the time of the full moon.

Elizabeth Hilton, of Liverpool, died on the 26th of October, 1760, aged 121, near the full moon.

John Constant died on the 16th of January 1763, aged 113, near the new moon.

On the 4th of July, 1826, died John Adams and Thomas Jefferson, the former aged 91 and the latter 83, within a few hours of the time of the new moon. At the time of the next full moon died Isaac Shelby, ex-governor of Kentucky, aged near 80. Benjamin Franklin, aged 84, died on the 17th of April 1790, at the time of the full moon. George Washington died of an acute disease, at the age of 68, on the 15th of December 1799, near the time of the full moon. Innumerable other facts to the same effect might be adduced.

That the moon, then, about the periods of her full and change, exerts an influence, in the production of disease, appears to be a truth, from which we are forbidden to withhold our assent. It is not a hypothesis, the offspring of abstract closet-speculation. It is the fruit of observation on what is passing around us. It is a plain every-day fact, of the existence of which all must be convinced, conditioned that they lay aside prejudice, and examine things as they are. We have the same evidence of it, that we have of the fact, that rain, in spring and summer, is favourable to vegetation. We see the rain falling, and shortly afterwards plants appear more healthy and vigorous, and their growth is more rapid. Nor can we attribute these phenomena to any thing else than the fostering showers. In like manner, we perceive that the occurrences of the new and full moon are accompanied by an increase in the amount of disease. Nor is this connexion

casual. It is uniform, and must, therefore, be that of cause and effect. The fact that disease occurs in greater abundance, about the full and change of the moon, than at other times, can be accounted for in no other way, than by ascribing it to the greater strength of lunar influence, at those periods.

In what way is this influence exercised?—Not *directly* on the persons of those who fall sick; but *indirectly*, through the medium of the atmosphere. What is the description of individuals, who chiefly experience its deleterious effects?—Invalids, and those of feeble constitutions, who may almost be denominated creatures of the atmosphere; whose health, at least, is much and easily injured by atmospherical vicissitudes. This is the case with all persons subject to chronic complaints, or who have strong predispositions to them. Changes in the atmosphere, especially from heat to cold, and from dryness to moisture, derange their health. But, as already stated, such changes occur, not only most frequently, but in greatest strength, at the times of the full and change of the moon. Hence most, perhaps I might say all constitutional complaints, of a chronic character, are more or less marked by paroxysms occurring at particular lunar periods; I mean at the full and change. It is on these principles only that we can account for the periodical attacks of hæmoptysis, asthma, and nephritic affections. They are chronic complaints, the suffering individuals are constantly predisposed to them, their constitutions being feeble are unable to resist strong deleterious impressions of any description, and hence disease is excited in them by the atmospherical vicissitudes that occur at the full and change of the moon. Whether these vicissitudes consist exclusively in changes in the *sensible* qualities of the atmosphere, or whether they may not have associated with them alterations in its *insensible* qualities, which may co-operate in the production of the general result, we have not facts to enable us to decide. There are not wanting, however, certain considerations, which somewhat favour the latter hypothesis.

During the prevalence of plague, epidemic yellow fever, or any other powerful epidemic complaint, all those, who are sub-

ject to the action of the febrile poison, are, for the time, in the character of invalids. They are more or less predisposed to the existing disease, and require, for its actual production, nothing but an adequate exciting cause. And this cause presents itself in the atmospherical vicissitudes, which occur about the full and change of the moon. Hence the multiplied attacks of the complaints at those periods. This multiplication is not to be attributed to an increase in the quantity or strength of the febrile miasm; but to a strong exciting cause, acting on those, whom the miasm has already predisposed to the disease. Any other exciting cause, sufficiently powerful, would produce the same effect, as certainly as atmospherical vicissitudes.

Individuals greatly enfeebled in constitution by old age are also invalids, suffering the infirmities, and predisposed to all the actual diseases, apoplexy, palsy, and others, that belong to their advanced period of life. To produce those diseases, therefore, nothing is required but an adequate auxiliary and exciting cause; and that again is found in the changes in the atmosphere, which occur at the times of the new and full moon. Hence the frequent occurrence, at those periods, of the specified complaints. And hence the further fact, that so many persons bowed down and shattered by the weight of years die at those periods. As an additional evidence of the influence of the condition of the atmosphere on such subjects, it is found, on inquiry, that a large proportion of them die during intensely hot and severely cold weather. All extremes of temperature and condition are deleterious to the enfeebled and unelastic constitution of old age.

These principles appear to explain the periodical recurrence of worm-fever. That disease attacks invalids, or those, at least, whose health and constitution are feeble and unresisting. In such subjects sudden changes in the atmosphere from warmth to coldness, and from dryness to moisture, repel the blood from the superficies and extremities of the body, and confine it to the deeper-seated parts. They produce in the system centripetal action, and a state of internal congestion, especially in the abdominal viscera, which are much debilitated. In this

deranged condition of things the intestinal canal participates, and its irritability is augmented in proportion to the degree of congestion which it sustains. For it is a law of the system, that an accumulation of *arterial* blood, adds to the irritability of the organ which contains it. On this heightened susceptibility the worms present act as an irritant, and produce fever, precisely as any other irritating substance would do. And, as already mentioned, were there no worms to produce the effect, the predisposition existing, in consequence of the increase of intestinal irritability, the fever would be excited by some error in diet, drink, or regimen. For, when the system is predisposed to disease, almost any act of excess, exposure, or imprudence, will produce an attack. Lunar influence, then, I repeat, is not thrown directly either on the human system, or the intestinal worms. At the periods of the full and change of the moon, it affects the atmosphere, in the manner heretofore stated; that affects the systems of enfeebled children from *without;* and then the worms, from the change produced, do mischief *within*. Were it not for this periodical increase of the irritability of the system, the worms, unless existing in very inordinate quantities, would be as harmless as common aliment. In corroboration of this, an excess of aliment injures a valitudinarian with no less certainty, than a superabundance of worms. I employ the term "superabundance" with a reference to the fact already stated, that, in all children, worms exist: and that they are rarely found in such excess as to do constant injury. Worms are very often blamed for mischief which improprieties in diet have produced. And there are but few cases, in which judicious treatment, as respects diet, regimen, and other points, will not restore health, without the employment of remedies technically called *anthelmintics*. It is to be understood, that these remarks relate more especially to affections attributed to two species of worms, the ascaris lumbricoides, and the trichuris.

Shall I be asked, whether there is not, in worm-fever, some peculiarity, which distinguishes it from all other febrile affections? I answer, that I know of none. It so completely re-

sembles other forms of fever, produced by gastric, or intestinal irritation, and in whose pathology abdominal congestion is a prominent feature, that no medical sagacity can distinguish it from them. Unless when he witnesses their discharge, no physician is authorized to pronounce confidently, that worms exist in the alimentary canal. No symptom, or combination of symptoms, on which he can fix his attention, affords him certainty. Strong probability is all he can attain. I still speak in reference to the ascaris lumbricoides and the trichuris.

What are the symptoms generally pronounced to be indicative of worms? They are as follows. An appetite variable, irregular, and sometimes unnatural, such as a craving for chalk, argillaceous earth, ashes, or charcoal; a fetor of the breath; acid eructations; pungent pain in the stomach and bowels; fulness and tension of the abdomen; slimy stools, accompanied with griping; a thickening of the upper lip; grinding of the teeth during sleep; picking of the nose; paleness of countenance, with a bluish cast under the eyes; short, dry cough; irregular pulse; occasional convulsions; fever marked by evening exacerbations; and a periodical aggravation of many, or most of these symptoms, about the full and change of the moon. Such, I say, are the leading phenomena, supposed to characterize worm-fever; yet there are none of them peculiar to it; there is not one of them that cannot be shown to appear occasionally in what may be denominated, and what really is denominated, bilious fever. Nor can worm-fever be cured without correcting vitiated abdominal secretions, and procuring natural and healthy ones, especially natural bile, any more than the remitting and continued fevers, which prevail in marshy districts, in the autumnal season. Indeed the condition of the system differs so little in those diseases, that, as relates to practice, it would scarcely be a mistake to call it identical.

Worm-fever, as every symptom of it shows, is the result of irritation and congestion in the abdominal viscera. While the congestion continues, the disease will continue. Remove the former, and the latter will disappear. How, then, is the congestion to be resolved? The answer is easy. It must be done

by secretion and revulsion; but chiefly by the former. And the secreting organs must be either those in which the congestion exists; or those in their vicinity. The secretory action of remote organs does not answer so well. I repeat here an observation formerly made, that natural and copious abdominal secretion cures what is called worm-fever, without the discharge of a single worm, while the discharge of worms, without the requisite secretions, does not. This clearly proves that secretion is the most important process.

The reader is, no doubt, by this time, prepared for the declaration from me, that, in the treatment of worm-fever, in general, I recognise no peculiarities. I say, "in general;" because, in some cases, where the worms are inordinately abundant, peculiarities are useful.

Is the temperature of the skin high, and the pulse full, strong, tense, and frequent? Is gastric irritation troublesome, and headache severe, and does delirium threaten, or already exist? The action is above the secreting point, and must be reduced by the lancet, or leeches placed on the epigastric region, or both, cold drinks, and cold water applied externally, especially to the extremities and the head; and it should never be forgotten, that cold applications to the back of the head, and along the sides, as low as the basis of the brain, act more promptly and powerfully, in reducing febrile excitement, than similar applications to the top of the head and the forehead.

The action of the system being reduced to the proper point, let secretory remedies be next employed; such, I mean, as may promote secretion in the abdominal viscera. Of these, the best are tartarized antimony and calomel, to be employed as in other febrile affections. The antimony may be given in doses either to puke or nauseate, as circumstances require; and on this point the physician must judge for himself. No universal rule can be given. Should these remedies produce copious secretions, of a natural appearance, the fact is encouraging; and by the same perseverance, and care in the use of them, that are required in the treatment of other febrile affections, the case will soon be brought to a favourable termination.

But, should the necessary secretions not be procured, and the febrile excitement rise again, recourse must be again had to the original means of reduction; or the antimony may be given in larger doses, more frequently repeated. To the calomel also may be added, according to the indication, rhubarb, jalap, or some of the active purgative gums. The object is, to excite natural bilious and intestinal secretion, and, for the attainment of that end, the means must be thus varied in adaptation to the condition of the system. There occur but few cases of worm-fever, that may not be, in this way, successfully treated. Nor need the practitioner trouble himself in general about the discharge of worms, provided he procure the requisite discharge of bile. In general, however, some of them will be discharged by this mode of treatment.

But are the worms so inordinately numerous, that they ought to be removed? For the attainment of that object, let the Carolina pink-root, the worm-seed oil of the shops, or a decoction of the seeds themselves, the cedar-apple, or the pulvis or limaturæ stanni, be exhibited in the usual way. Those articles so sicken and debilitate the worms, that, by subsequent purging, shey are much more easily and certainly discharged. But, while the fever is high, these "vermifuges" should never be administered. If they are, they will increase excitement and strengthen the disease. Previously to their exhibition, therefore, let the fever be greatly reduced, if not entirely removed, by the mode of treatment already indicated. From the neglect of this, much mischief has been frequently done. Nor is the neglect rare. On the contrary, it is frequent, if not general. Of this the reason is obvious. The worms are considered the sole cause of the fever; and hence their removal is the object, at which alone the practitioner aims. He does not believe, that, without their evacuation, the fever can be subdued. His first remedy, therefore, is a dose of "anthelmintics." When the febrile affection is slight, this practice is not very dangerous, and may prove successful. But in cases of high excitement, it is exceedingly injurious.

Such is the outline of the treatment of a paroxysm of worm-

fever. But the disease being periodical, another paroxysm is to be looked for, about the time of the first full or change of the moon. The prevention of that, therefore, is the next object of the practitioner. And it is an important one. How is it to be attained? I answer, by pursuing a course of practice founded on the principles, which apply to the prevention of a paroxysm of any other periodical complaint, especially where the paroxysms recur at long intervals.

In all such complaints, the systems of the patients are, at different times, in very different conditions. During the existence of a paroxysm, the disease is acute and severe; but, in the interval between one paroxysm and another, it is chronic, and comparatively slight. Accommodated to these two different states, must be two different schemes of practice, one acute, the other chronic. For a chronic disease requires chronic treatment; and the reverse.

But, in all cases, chronic practice consists much more in the judicious management of diet and regimen, than in the administration of medicinal substances. Nor is there any complaint, in which an adherence to this rule is more essential, than in that of worms.

In the intervals between the febrile paroxysms, therefore, let the diet be easy of digestion and nutritious, but moderate in quantity. And, as relates to different individuals, a knowledge of the digestibility of food must be the result of experience. Articles of diet suitable to some, are often found prejudicial to others. On no account let the stomach be overloaded. An excess in quantity and improprieties in quality are alike injurious. The effect, in each case, is to continue or increase the debility of the chylopoetic organs, and thus maintain the predisposition to disease. In the mean time, let regular exercise be taken in the open air, sufficiently active to promote free circulation, but not so violent as to produce fatigue. An attention to clothing is also important. To keep up the requisite cutaneous action, let flannel be worn next to the skin, while the outer garments are light and easy, and of a fabric calculated to preserve the warmth of the body. To

guard the system generally, but especially the extremities, against the effects of cold and humidity, no precaution should be omitted. Nor can too much attention be paid to the preservation of a regular state of the bowels, and the procurement of healthy abdominal secretions. In effecting this, diet and regimen do much; but medicinal substances may be occasionally requisite. Of these, calomel and the blue pill, aided, if requisite, by rhubarb or aloes, are usually the best. Others may be preferable under given circumstances, of which the practitioner must judge. In these cases, a course of sulphur has been found useful. While it regulates the bowels, it contributes also to the healthy secretory action of the skin, and mucous membranes.

It is in the treatment of chronic affections alone, that the class of remedies, called tonics, is admissible. In convalescence from acute diseases, that have been properly treated, they are never necessary; and, from injudicious administration, often do mischief. In the interval between one paroxysm of worm-fever and another, they are much recommended, and may, perhaps, in some cases, do good. They consist chiefly of the various vegetable bitters, one or two of the mineral acids, chalybeates, and lime-water. The latter remedy is generally supposed to produce its salutary effects, by dissolving the superabundant intestinal mucus, which serves as a nidus, and perhaps also as food for the worms. But this is an error, arising from a want of physiological knowledge. Lime-water does not dissolve the mucus of the alimentary canal. If it did, it would probably do mischief, by bringing the worms more immediately into contact with the denuded mucous membrane. The very object of the secretion of a superabundance of mucus, in cases of an inordinate prevalence of worms, would seem to be the protection of the intestines from excessive irritation. If lime-water does any good, it is by exciting the chylopoetic system to healthy action. But, in worm-fever, as well as in every other disease, by far the best tonic consists in a well directed and well sustained course of dieteticks and general regimen.

The ascarides vermiculares, which constitute the third species of round worms, are not generally accompanied by so much constitutional derangement, as the other two. Their existence is indicated chiefly by a troublesome itching and burning in the rectum, near the anus, and by a frequent discharge of themselves. Sometimes they voluntarily make their escape, per anum, and are thus detected. I do not know that the complaint, which accompanies them, is marked by exacerbations at lunar periods.

In the removal of this species purging has but little effect. Recourse is, therefore, had to injections. The most effectual of these are composed of sweet oil or castor oil, mercurial ointment, spirits of turpentine mixed up in a solution of gum-arabic, or some other mucilaginous fluid, and a strong decoction of Carolina pink-root, or a weaker one of tobacco. Here also, as in the case of the ascaris lumbricoides and trichuris, the restoration of general health, by a strict attention to diet and regimen, removes the worms.

In the United States, the disease of the tape-worm is not very common. I have therefore but rarely seen it. Its chief diagnostic symptom, next to its actual appearance, is a peculiar feeling, in the intestines, as if some body, extending along a considerable portion of them, was rolling itself round in them. This complaint, like that accompanying the round worms, is said to be marked by lunar exacerbations. If so, it must be for the same reason. The patient being an invalid, his system makes but a feeble resistance against sudden and great vicissitudes in the atmosphere. Centripetal action, leading to abdominal congestion, and an increase of irritability is the consequence. In this condition of things, the augmented verminous irritation gives rise to a paroxysm of fever.

On the treatment of this disease, I have but little to say. Sulphur, a strong decoction of Carolina pink-root, and terebinthinates, accompanied, or rather followed by active purgatives, are recommended on good authority. So is an amalgam of zink and quicksilver, succeeded by energetic purging. As connected with this latter remedy, the following case is cu-

rious and interesting, and not without importance, in a practical point of view.

In the year 1818, J. P., of Louisville, was attacked severely by the tape-worm. After the employment of sundry remedies, by which portions of the worm were, from time to time, expelled, he was advised to the use of an amalgam, composed of equal parts of zink and quicksilver. Of this he was directed to swallow, every hour, a dose of an *ounce-weight*, until he had taken *twenty-four doses*. He commenced, intrepidly, the formidable process, and proceeded in it regularly, until he had swallowed sixteen ounces of the amalgam, in sixteen hours. By this time, an uncomfortable and increasing weight (he called it very correctly, "a dragging down") in his bowels, admonished him to stop. He did so; and, on the second day afterwards, took an active purgative, by the aid of which he discharged a much larger portion of the worm, than he had ever done before. But, of the amalgam, he discharged very little. By means of repeated cathartics, taken subsequently, at periods of time not very remote from each other, he conceived, at length, that the worm was entirely removed; and that he was now well, except that almost the whole of the amalgam was still in his bowels. Feeling no great inconvenience from this, he made a voyage to New-Orleans. Perceiving, during his absence, that he still retained some remains of the worm, he swallowed, on his return to Louisville, in four successive hours, four ounces more of the same amalgam, having now taken, in all, *twenty ounces*.

From the worm-affection he was soon afterwards relieved; but not from that produced by the remedy. A large portion of the metallic mass still remained in his bowels, and began to excite in them considerable uneasiness.

In the summer of 1820, I saw him for the first time. His health was much shattered, his person emaciated, his countenance sallow, and his whole appearance strikingly hypochondriacal. The amalgam had been in his bowels nearly two years. When he related to me his case, I considered the story a morbid fiction, and believed his intellect somewhat deranged.

But, on further examination, I discovered my mistake. Near to the umbilicus was a hard, round, floating lump, of considerable size, which, by the requisite management, I could grasp in my hand, and which appeared to weigh about a pound. The patient himself was confident it weighed more, for he asserted, that, of the twenty ounces of amalgam swallowed, he had never discharged more than two.

The account he gave me, of the effects of this moveable ball on him was, that, if, in walking, he made a false step, or, in any other way, jarred himself, it gave him great uneasiness; and that it entirely disqualified him for riding on horseback. When he attempted to exercise, in that way, it created, by its irritation, a constant and painful *erectio penis*. He could not, without inconvenience, bear even the motion of the easiest carriage. He was exceedingly anxious for the removal of the foreign body, and urged its extraction, by a surgical operation, if he could not be relieved from it, in any other way. From this resolution he was dissuaded, for the present, and advised to bear with his misfortune, as long as it should be tolerable, reserving its removal, by a surgical operation, for the last extremity. In the mean time, a suitable diet and regimen were prescribed, with the use of occasional laxatives or purgatives, as circumstances might require, with a view to guard, as much as possible, against the bad effects of intestinal irritation.

In the autumn of 1823, the patient died of the malignant fever, which prevailed that season in Louisville. The metallic mass had now been in his bowels nearly five years. On opening his abdomen, the quantity found amounted to about eight or ten ounces. My belief had been, that it was in one of the cells of the colon, where nature had formed a cyst around it to retain it, and keep it from moving along the intestine, analogous to the capsule, which she throws around a leaden bullet, when buried in muscular or cellular substance. But I was mistaken. Its situation had been in the cœcum; and, after death, or on the near approach of it, the cyst had given way, and it had fallen to a lower position in the abdomen. From

this singular case, the following inferences appear to be deducible, and may be useful in practice.

1. In the treatment of the disease of tæuia, an amalgam of zink and quicksilver is a valuable remedy.

2. In the case of J. P. the quantity given was too large.

If, instead of an ounce every hour, the article had been given to the amount of an ounce a day, in four equal doses, for two or three days, and then followed by active purging, and the process repeated as requisite, the probability is, that it would have removed the worm, without injuring the patient. In similar cases, such treatment may be warrantably recommended. I shall only add, that, in preparing this article, my chief object has been, to contribute toward the demolition of such nosological distinctions between febrile affections, as I believe to be unfounded; and which lead occasionally to injudicious practice.

Art. IV.—*Notices of Western Botany and Conchology.* By C. W. Short M. D. and H. Hulbert Eaton A. M. (r. s.)

HAVING made during the last fall, in company together, an excursion from this place to the Ohio river, a distance of about eighty miles due north from Lexington, sometimes travelling by stages, and sometimes on foot; we had a fair opportunity and abundant leisure for examining the vegetable productions of the intervening country; and also the shells of the Ohio, and the contiguous portion of the great Miami river. Of these, as we have thought it might not be uninteresting to naturalists in other portions of the country, we subjoin a catalogue of each, restricting it to those which were found on this excursion; with a few desultory remarks. It may not be amiss to observe, as accounting for the paucity of plants found in bloom, that the summer and fall had been unusually hot and

dry, and that, in many portions of the western country, the grasses and other small herbaceous vegetation were literally burnt up by the drought. The catalogue may, nevertheless, be regarded as a tolerably fair specimen of the latest autumnal Flora of this section of the Union—no plant being mentioned which was not found in flower, and none having bloomed subsequently, in consequence of the occurrence of severe frosts, before our return from the excursion, which commenced on the 16th of September, and terminated on the 1st of October.

The aridity of the season, if it lessened the number of botanical acquisitions, proved, on the contrary, eminently favorable to conchological researches; as the waters of the Ohio and Miami rivers were much lower than they had been for many years preceding, and consequently afforded an opportunity of examining their shores some distance into these streams. The particular localities which were especially and very carefully examined, for shells, were those portions of the Ohio river, on the northern side, lying a mile or two above and below the mouth of Muddy-creek, (a small stream which empties into that river about fifteen miles below Cincinnati,) and the eastern borders of the great Miami, contiguous to the village of Cleves, in Hamilton county, Ohio.

I. PLANTS.

Lycopus Europæus. } Both met with occasionally on the moist
Lycopus Virginicus. } rocky banks of creeks, &c.

Collinsonia Canadensis. (Var. ovata.) Found in the flat moist woods of Ohio, above the North Bend.

Commellina communis. In abundance on the alluvion borders of Eagle creek at the crossings; and on Muddy-creek, Ohio.

Schollera graminifolia, W. In the low waters of the Miami, at the Cleves ford; rare.

Ceresia fluitans. Moist woods, Ohio.

Isnardia palustris, W? (Ludwigia apetala Walt. L. nitida, Ph. and Mx. L. palustris, Ell.) On the marshy borders of the

Ohio river, at the mouth of Muddy-creek. Although this plant is placed by the most of botanists among the apetalous species of Ludwigia, the specimens here found had distinct petals of some size, and a purple colour.

Chenepodium botrys. Abundant on the sandy banks of the Ohio every where: its odour highly fragrant.

Chenepodium anthelminticum. (Wormseed.) Equally abundant in some situations on the river, with the former.

Gentiana quinqueflora. (G. amarelloides, Mx.) In the woods around Big-bone lick—not frequent.

Gentiana saponaria. Found in a single locality on Muddy-creek.

Impatiens pallida, N.
Impatiens fulva, N. } Both of these species of Touch-me-not occur abundantly, and in equal profusion, in all damp shaded and rich soils throughout the western country, especially among the drift-wood and debris of creeks; and they continue to flower and ripen seed from an early period in the summer, until the frosts destroy them.

Lobelia syphilitica. Occurs abundantly in neglected pastures around Lexington.

Lobelia inflata. In equal profusion and in similar situations.

Lobelia cardinalis. A solitary specimen of this beautiful species was found in bloom, at this late season, on a branch of Big-bone creek.

Onosmodium hispidum. On a clayey hill-side near the tavern at the Big-bone lick: rare and nearly out of bloom.

Elodea petiolata. Ell. (Hypericum petiolatum, Walt.) Ohio bank.

Mimulus ringens.
Mimulus alatus. } Both in abundance on the moist alluvion of Muddy-creek and other similar situations.

Mentha borealis.
Mentha tenuis? } On the rocky beds of Eagle and Big-bone creeks:—the former species much most common.

Gerardia tenuifolia, W. (G. erecta, Vahl.) On the summit of the Dry-ridge near Jones's:—the only locality in which we have met with it in the western country.

Verbena hastata. Banks of the Ohio river, common.

Zapania nodiflora. First met with on the dry gravelly beds of small streams, and afterwards, in much greater luxuriance, on the banks of Muddy-creek.

Capraria multifida. Big-bone creek, and the sandy borders of the Ohio river.

Lindernia attenuata. Mouth of Muddy-creek.

Chelone glabra. (Var. *purpurea*) In a marsh on Muddy-creek, in company with Gentiana saponaria.

Hyssopus nepetoides. Frequent in pastures and meadows on the Ohio river.

Scrophularia marilandica. Abundant in the corners of fences and waste grounds, throughout the Western country.

Erysimum palustre. Mouth of Muddy-creek.

Cleome dodecandra, W. (*Polanisia graveolens*, T.) Abundant on the gravelly margin of the Ohio river; nearly out of bloom at this time.

Strophostyles angulosa. Abundant among the sand and gravel of the Ohio borders, growing quite to the water's edge.

Rudbeckia triloba. In the oak-woods about Gaine's—rather rare.

Rudbeckia fulgida. A common yellow-flowering weed; abundant in fence corners throughout the fall.

Coreopsis tricosperma. A very rare and pretty plant,—a single specimen only was found, on Muddy-creek.

Bidens bipinnata. A small kind of Spanish-needle; common.

Bidens chrysanthemoides. This pretty species of an unsightly genus is abundantly met with throughout the western country, in all moist situations; its large, showy, yellow flowers making a very handsome appearance at this late season of the year.

Actinomeris squarrosa. A tall weed, common in the fence rows of cultivated fields.

Conyza camphorata. Quite common on the ridge road. The specific name is surely a very inappropriate one, its smell being much more similar to the peculiar odour of the negro than to that of camphor.

Eupatoreum perfoliatum. (Thoroughwort &c.) Exceedingly common in all marshy situations.

Eupatoreum cælestinum. This beautiful plant first occurred to us on the head-waters of Big-bone creek; and afterwards in profuse abundance on the higher banks of the Ohio river.

Aster conyzoides. And several other Asters not accurately determined.

Prenanthes ————? Ohio bottoms.

Gnaphalium uliginosum. Big-bone Lick.

Helenium autumnale. (Sneeze-weed.) Every where abundant, especially about water-courses.

Eclipta procumbens. Frequent on water-courses, sometimes being strictly procumbent in its growth, and again entirely erect.

Acnida cannabina. (Water hemp.) Common on approaching the Ohio river.

Ambrosia trifida. (Horse-weed.) Very common every where in waste grounds.

Asplenium angustifolium. On the high table lands between the Ohio and Miami rivers: rare.

II. SHELLS.

As already observed, our principal examinations for shells were made near the mouth of Muddy-creek in the Ohio river, and near Cleves in the great Miami river; in this list we shall refer to these two localities as Ohio and Miami. A fact well worth relating, is, that the shells from the Miami are almost invariably much larger than those of the same species from the Ohio; and what is very remarkable, are rarely or never in the least degree decorticated even on the beaks; and several species are found there, free from erosion, which are commonly described as possessing decorticated beaks. The shells from the Miami are generally covered with a greyish, granulated, calcareous crust, deposited from the water—which, running over Geodiferous Limerock, always contains more or less carbonate of lime in solution.

In the few remarks which we shall make in relation to the Naiades, we shall follow Cuvier in considering that margin of

the shell anterior which contains the mouth, and which always moves foremost when the animal is in a progressive state—it being the margin towards which the beaks of the shell incline. In stating the dimensions of the shells, the figures always refer to inches and decimals of inches.

At the commencement of the list of the species of each genus, enough of the generic characters are given to distinguish our four genera from each other, and no more.

UNIO. *Valves free, having one or more cardinal and lateral teeth in each.*

Unio plicatus, LE SUEUR. (U. costata of Raf., is probably a variety of this species. See Raf., p. 49, pl. 82, fig. 13 and 14.*) Ohio and Miami: rather common. The undulating surface and the thickness of the different specimens of this species vary so much as to offer some apology for Barnes's subdivision of it into three species, (plicatus, crassus and undulatus.) The folds are nearly wanting in some specimens, while in others they are very profound. This shell was observed from half an inch to five inches in breadth. We have one specimen from the Miami with beaks perfectly entire and white; its dimensions are as follows:—Diameter 2. 0, Length 3. 2, Breadth 4. 2, while a young one from the Ohio is eroded, and measures—Diameter . 6, Length . 8, Breadth 1. See Am. Jour. Sc., Vol. 6, pages 118—120, pl. 1, 2 and 3.

Unio mytiloides, RAFINESQUE. (U. undatus, Banes.) Ohio and Miami: most abundant species in the Ohio. This shell varies much with regard to several particulars: sometimes there is a deep sulcus running from the beaks to the posterior basal margin, while other shells have no sign of a furrow. The nacre varies from white to perfect pink: the lateral tooth of the right valve of the pink variety is sometimes *treble*, and in some of the white ones it is hardly double. Very young shells are often marked with brown rays, particularly on the beaks and over the umbo. See Am. Jour. Sc., vol. 6, p. 121, pl. 4, fig. 4.

Unio scalenius, RAF. Ohio and Miami: common. The spe-

* We refer to a work "on the bivalve fluviatile shells of the Ohio river" by S. C. Rafinesque; published in 18[illegible].

cimens from the Ohio are very much decorticated on their beaks, but those from the Miami are always perfectly entire. The beaks of the Ohio shells are terminal and projecting, while those from the Miami are not terminal; yet there is no doubt of their identity. See Raf., p. 43, pl. 31, fig. 24 and 25.

Unio ellipsis, Lea. Ohio: great abundance. Sometimes beautifully radiated with green. See Lea's Naiades, p. 10, pl. 4, fig. 4.

Unio sulcatus, Lea. Ohio: rather rare. Our specimens are all more or less purple within, none of them being as represented by Mr. Say, "pearlaceous-white." See Lea's Naiades, p. 44, pl. 8, fig. 12; and Am. Con., vol. 1, No. 1, pl. 5, lower figures.

Unio ridibundus, Say. Ohio and Miami: rare. This beautiful little shell, first described, with his usual minuteness and accuracy, by Mr. Lea, as a variation of his *U. sulcatus*, has since been made a new species by Mr Say, and described and figured by him in the first number of his "American Conchology," plate 5, upper fig. All of our specimens are more or less purple within—some very much so.

Unio torulosus, Raf. (U. cornutus Barnes; U. foliatus, Hildreth.) Ohio and Miami. All possible varieties of this heteramorphous shell were found in the Ohio: the *U. foliatus* of Hildreth, which Mr. Lea thinks nothing more than a variety of the *U. cornutus* of Barnes was found, and among our numerous specimens of this variety, not one had hardly the rudiment of a horn. In the variety described and figured by Rafinesque, the tubers begin to appear, so as to render the shell *torulose*. Neither *torulosus* nor *cornutus* will apply to every variety; but since Mr. Rafinesque's name was given first, it must be adopted, and that of Mr. Barnes, although generally used, laid aside. See Raf., p. 38, pl. 32, fig. 11 and 12; and Amer. Jour. Sc., vol. 6. p. 122, pl. 4, fig. 5; and Lea's Naiades, page 32, note.

Unio verrucosus, Barnes. Both varieties, *purpureus* and *albus*, occur plentifully in the Ohio. The *warts* are more or less numerous, and in some instances almost entirely wanting. See Am. Jour. Sc., vol. 6. p. 123, pl. 5. fig. 6.

Unio metanever, RAF. (U. nodosus, and rugosus, Barnes.) Ohio: rare. See Raf., p. 39, pl. 81, fig. 15 and 16; and Am. Jour. Sc., vol. 6, pp. 124 and 126, pl. 6 and 7.

Unio lachrymosus, LEA. Ohio and Miami: very rare, we only finding three specimens. A very large specimen from the Miami measures—Diameter 2. 8, Length 3. 3, Breadth 4. 1. The epidermis in this remarkably large specimen is brownish-yellow with a very slight tinge of olive in some places: the beaks are perfectly entire: the anterior, inferior, and posterior-inferior portions of the disk of the shell are smooth and free from tubercles. See Lea's work, page 8, pl. 6, fig. 8.

Unio tuberculatus, BARNES. Ohio and Miami: rather rare. One from the Miami with the beaks free from decortication, measures—Diameter 1. 35, Length 2. 65, Breadth 4. 7.

Unio cylindricus, SAY. Ohio: rather rare. See Nich. Enc., vol. 4, pl. 4, fig. 3; and Am. Jour. Sc., vol 14, page 282, pl. 2, fig. 13.

Unio cariosus, SAY. (U. crassus, Say; U. carinatus and ellipticus, Barnes.) Ohio and Miami: very abundant. Some of the specimens of this species were more ponderous than any other found during this excursion. This is also a polymorphous shell: some are nearly circular, some quadrangular, some ovate, and some almost perfectly elliptical. The color of the nacre varies from perfect salmon to pure white: the epidermis is of all shades from dark brown to light yellowish. All the shells from the Miami have perfectly entire white beaks, and not being in the least degree *carious* are not entitled to the specific appellation it bears. From this it appears evident that the *decortication* of the beaks of shells ought not to be mentioned as an *essential* character. The dimensions of three of the Miami shells follow:

Diameter 1. 9, Length 3. 3, Breadth 5. 2.
do. 2. 1, do. 3. 3, do. 4. 7.
do. 1. 7, do. 2. 5, do. 2. 8.

See Nich. Encyc., vol. 4, pl. 2, fig. 7, and pl. 1, fig. 8; and Am. Jour. Sc., vol. 6, p. 259 and 271, pl. 11, fig 10, and pl. 13, fig. 19.

Unio phaseolus, Hildreth. (U. planulatus, Lea.) Ohio and Miami: rather rare. This shell was first accurately figured by Mr. Lea, and described by him under the name of *U. planulatus*, but, (as very justly observed by Mr. Peter of Pittsburgh,) as Dr. Hildreth's name has the priority it must be adopted. Some specimens are arcuated on the basal margin as represented by Dr. Hildreth, but more commonly slightly curved or straight. The dimensions of one of our shells is, Diameter 1. 1, Length 1. 7, Breadth 2. 9. See Lea's work, page 45, pl. 19, fig. 13; and Am. Jour. Sc., vol. 14, p. 283.

Unio securis, Lea. (U. depressa, Raf.) Ohio and Miami: not very abundant. A specimen from the Miami measures—Diameter 1. 5, Length 2. 8, Breadth 3. 7. The posterior basal margin of this large specimen is slightly arcuated, and in several specimens it is straight.

Unio torsus, Rafinesque. Ohio: in great profusion. Some specimens are rendered emarginate posteriorly by a broad furrow running from near the beaks to the posterior margin. Lateral teeth generally double in both valves. See Raf., p. 45, pl. 82, fig. 1, 2 and 3.

Unio circulus, Lea. Ohio: common. It was suggested to us by Mr. Peter that this shell was the *U. orbiculatus* of Dr. Hildreth, but we are unable to say whether the suggestion be correct; and if it is, it is highly probable that his name will not be retained, for Rafinesque has probably described it before him under the name of *U. subrotundus*; if so, this latter name is the one to be appropriated to the shell. The color of the nacre varies from perfect white to beautiful flesh color or pink. Some of our specimens are emarginate, on account of sulci running from the beaks to the posterior basal margins. See Lea's work, page 47, pl. 9, fig. 14; Amer. Jour. Sc., vol. 14, p. 284; and Raf., p. 42, pl. 81, fig. 21, 22 and 23.

Unio irroratus, Lea. Ohio and Miami: rather common. See Lea's work, p. 11, pl. 5, fig. 5.

Unio rectus, Lamarck. (U. prælongus, Barnes.) Ohio and Miami: common. This species occurs of all sizes, with the nacre either clear and white, or (in young specimens) beauti-

fully colored, either entirely or only in the centre of the cavity of the umbo, of a lively purple, pink or salmon. In some instances the cardinal teeth are very small, while in other shells they are thick and prominent, and often beautifully serrated. A large one measures—Diameter 1. 5, Length 2. 3, Breadth 5. 5. See Am. Jour. Sc., vol. 6, p. 261, pl. 13, fig. 11; and vol. 14, p. 286.

Unio gibbosus, BARNES. Ohio: common. Some of our specimens are rather long, with the basal margin arcuated; others are shorter, and with a straight basal margin. See Am. Jour. Sc., vol. 6, p. 262, pl. 11, fig. 12.

Unio cuneatus, BARNES. Ohio and Miami. A specimen from the Miami, with entire white beaks, measures—Diameter 1. 6, Length 2. 5, Breadth 3. 6. See Am. Jour. Sc., vol. 6, page 263; and vol. 14, p. 279, pl. 1, fig. 3.

Unio ovatus, SAY. Ohio, but far more abundant and of a larger size, in the Miami; one measures—Diameter 2. 5, Length 3. 8, Breadth 5. 4. A few small specimens were found in the Ohio beautifully radiated, thin and exceedingly fragile: one of these measures—Diameter 1. 1, Length 1. 6, Breadth 2. 1. The posterior slope of the large specimens is far less flattened than that of the smaller ones. See Nich. Encyc., vol. 4, pl. 2, fig. 7; and Am. Jour. Sc., vol. 14, p. 287, pl. 2, fig. 21.

Unio occidens, LEA. Ohio and Miami: not very abundant. See Lea's work, page 49, pl. 10, fig. 16.

Unio triangularis, BARNES. Ohio and Miami: common. We found two varieties of this shell; one figured by Barnes, measuring—Diameter 1. 2, Length 1. 0, Breadth 1. 8; in this the cavity of the beaks and umbo are very deep: the other figured by Say, measures—Diameter 1. 1, Length 1. 8, Breadth 2. 4; the cavity of the umbo and beaks of this variety are not much over half as deep as in the first. The latter variety occurs more frequently in the Miami, and in that locality the beaks are never eroded. See Am. Jour. Sc., vol. 6, p. 272, pl. 18, fig. 17; and Am. Con., vol. 1, No. 1, pl. 4.

Unio parvus, BARNES. Ohio: rare. The smallest American species. Shell thick. Diameter .4, length .6, breadth 1. 0.

Old specimens are generally very much eroded, while young ones are nearly entire on the beaks. See Am. Jour. Sc., vol. 6, p. 174, pl. 13, fig. 18.

Unio zig-zag, Lea. Ohio and Miami: common. A beautiful little shell; when full grown larger and thinner than the U. parvus. See Lea's work, page 54, pl. 12, fig. 19 and 22, a.

Unio triqueter, Rafinesque. (Unio calceolus, Lea.) Miami: rare. Mr. Rafinesque having described and figured this shell under this name first, is entitled to it. Mr. Lea's specimens were very young, yet there is no doubt that his shell and ours are the same. The beaks are entirely free from decortication, and invariably present two or three deep transverse folds: the substance of the shell is thin and fragile until it attains to nearly its full size, when it becomes near one eighth of an inch in thickness. The *posterior dorsal* portion presents slight longitudinal folds, which, going to the margin of the shell, in some instances cause it to be crenulated: basal margin is either slightly curved, straight or arcuated on its posterior half. The posterior slope is much more flattened than represented by Lea, a prominent ridge passing from the beaks to the posterior angle of the shell. In our specimens the epidermis is olive-yellow, either light or dark, with numerous dark green diverging rays nearly covering the whole disks; in old specimens the rays are so far interrupted by the wrinkles as to appear in the form of radiating square spots. None of our specimens have any appearance of a lateral tooth, except the smallest, in which it is but rudimentary, and is far less prominent than in an *Alasmodonta*, so that there would be some propriety in removing this shell to that genus. The nacre is bluish-white, and in the youngest, the dark exterior rays are visible through it. The dimensions of the largest and smallest of our shells—

Diameter 1. 6, Length 2. 2, Breadth 3. 5,
do. 1. 1, do. 1. 5, do. 2. 7.

See Raf., p. 34, pl. 31, fig. 1, 2 and 3; and Lea's work, p. 7, pl. 1, fig. 1.

Unio monodonta, Say. (Alasmodonta costata, Rafinesque.) Ohio; rather rare. This shell is evidently intermediate be-

tween the Unio and Alasmodonta, and argues something in favor of Lea's proposed division of our Naiades into two genera. Diameter 1. 0, Length 1. 6, Breadth 4. 5. See American Conchology, vol. 1, No. 1, pl. 6, and description; also Raf., p. 52, pl. 82, fig. 15 and 16.

SYMPHYNOTA.* *Valves connate.*

Symphynota alata, LEA. (Unio alatus, Say.) Ohio and Miami: great abundance, particularly in the latter river; some of which were of enormous size—measuring eight inches in breadth, with entire beaks. Nacre always purple. See Nich. Encyc., vol. 4, pl. 4, fig. 2; Am. Jour. Sc., vol. 6, p. 260; and vol. 14, p. 285, pl. 1, fig. 17; and Lea's work, p. 62.

Symphynota complanata, LEA. (Alasmodonta complanata, Barnes.) Ohio and Miami: rare. Valves more compressed than the S. alata: nacre white. See Am. Jour. Sc., vol. 6, p. 278, pl. 13, fig. 22; and Lea's Naiades, p. 62.

Symphynota lævissima, LEA. Ohio: rather rare. A very delicate highly polished shell, connate before and behind the beaks. See Lea's Naiades, p. 58, pl. 13, fig. 23.

Symphynota gracilis, LEA. (Unio gracilis and planus, Barnes.) Ohio and Miami: rather common. Some specimens from the Miami were rayed with green, and some with brown. See Am. Jour. Sc., vol. 6, pp. 274 and 272, pl. 13, fig. 16; and Lea's work, p. 66.

Symphynota tenuissima, LEA. (Anodonta lata, Raf.?) Ohio and Miami: rather rare. An exceedingly thin fragile shell. One

* Whether Mr. Lea is to be followed in his division of the Naiades is a question to be decided by other Naturalists; and it is not now our province to criticise particularly the advantages and disadvantages resulting from the division, yet a few remarks may not be improper. Objections can be raised against either arrangement: we find some shells which seem to connect the Unio with the Alasmodonta, for example the U. monodonta of Say (A. costata, Raf.) is intermediate, and in some specimens there is no appearance of lateral teeth, and sometimes it even approaches the Anodonta: and the U. triqueter of Rafinesque (U. calceolus, Lea) in old specimens is certainly a true Alasmodonta, it having no lateral teeth, while young specimens shew it to be a real Unio. Nor would the difficulty be entirely done away by following Mr. Lea; for some of his connate shells are often found free, when they would appear to be perfect. There is some doubt respecting the *U. monodonta* of Say, for we have some young specimens that appear to have been slightly connate.

specimen measures—Diameter . 9, Length 1. 8, Breadth 3. 4. See Lea's Naiades, p. 67, pl. 11, fig. 21; and Raf., p. 51, pl. 82, fig. 17 and 18.

ALASMODONTA. *Valves free, with one or more cardinal and no lateral teeth.*

Alasmodonta rugosa, Barnes. Found in great abundance and of enormous size in the Miami, and sparingly in the Ohio. The Miami shells have entire white beaks, while the Ohio shells are decorticated. Some specimens are rayed: nacre from pale salmon to bluish-white. A large specimen measures Diameter 1. 8, Length 2. 9, Breadth 5. 1: and a young specimen from the Ohio measures—Diameter . 4, Length . 9, Breadth 1. 8. It is possible that this may prove to be a symphynota, for in some specimens the ligament is almost concealed by the projecting edges of the dorsal margin which appear as though they might have been united. See Am. Jour. Sc., vol. 6, p. 278, pl. 13, fig. 21.

ANODONTA. *Valves free, without teeth.*

Anodonta undulata, Say? Ohio and Miami: rare. Those from the Miami have entire beaks, but those from the Ohio have them eroded. Our specimens are so much larger than the one figured and described by Say, as to leave us a little in doubt as to the certainty of our having his shell. Our specimens have a brown epidermis, but agree in all other particulars with Mr. Say's description, especially when he says "in the left valve, immediately under the beak, the margin is curved inwards for the reception of a corresponding marginal projection of the opposite valve." Two specimens measure as follows:

Diameter 1. Length 1. 7, Breadth 2. 9,
do. . 75, do. 1. 2, do 2.

See Nich. Encyc., vol. 4, pl. 3, fig. 6, (a young one?)

In Lamarck's family—"*Conques*"—we only found an individual species.

Cyclas similis, Say. Ohio: common. About the size of a cherry stone, though sometimes even half an inch broad. See Nich. Encyc., vol. 4, pl. 1, fig. 9.

As we paid but little attention to the collection of univalve shells, our list will be small.

Melania Sayii, WOOD. Ohio: very abundant. We have seen no description of this shell, but it is readily distinguished by a broad deep furrow passing around the centre of the whorls, with a ridge bounding each side: the lower ridge and a portion of the furrow are successively concealed by new volutions, the *body whorl* alone showing the entire furrow. It is figured by Mr. Wood in his "Supplement to the Index Testaceologicus," pl. 4, fig. 24, (under Stronbus;) and named on page 42 of the same work.

Melania Virginica, (Lymnæa Virginica, Say.) Ohio and Miami: very common. A smaller and smoother shell than the last. See Nich. Enc., vol. 4, pl. 2, fig. 4, and Say's description, article Conchology.

Paludina decisa, SAY. Ohio and Miami: common. Some very large ones were found—more than an inch and a half in length. Epidermis varies from dark brown to light greenish-yellow. See Am. Con., vol. 1, No. 1, pl. 10, upper and lower figures.

Paludina? (Perhaps poor specimens of P. vivipara.) Ohio: rather common. Our specimens appear intermediate between Melania and Paludina, yet they are so much eroded that we cannot say to which our shell belongs. See Am. Con., vol. 1, No. 1, pl. 19, middle figures.

SUMMARY OF THE SHELLS.

We collected of the	*Unio*	28	species.
	Symphynota	5	—
	Alasmodonta	1	—
	Anodonta	1	—
	Cyclas	1	—
	Bivalves		36,
	Melania	2	—
	Paludina	2	—
	Univalves		4,
Total number of Species collected			40.

ART. V.—*Remarks on Struma Africana, or the disease usually called Negro Poison, or Negro Consumption.* By LUNSFORD P. YANDELL M. D. of Nashville, Tennessee.

THE unity of the origin of the different varieties of the human race has long been a vexed question, which, although ably argued pro and con, may safely be pronounced still unsettled. By many the investigation of the subject is regarded as altogether idle, while others look upon it as something worse. Not so. The Bible was evidently intended as a code of morals, to ameliorate the heart, to teach erring man the will and attributes of his Maker; and not to afford a knowledge of Natural History, or indeed of any other branch of physical science. We have some striking and instructive examples on record, of the folly of making the truth of such points in natural science an *experimentum crucis* of the truth of Revelation. The researches of Gallileo were considered by the orthodox of his time as impious in the extreme; and it is known that the penalty which he paid for the temerity of promulging his discoveries in Astronomy—discoveries which are deemed, at the present time, of unequivocal lustre and importance, was the forfeiture of his native country. The theories which were then pronounced to be in direct contravention of scriptural doctrine, and of every thing dear in the Christian scheme, are now received by all classes alike, as established science. This should teach us caution in resting the whole validity of Revelation upon a single thread, against which fresh arguments, it may be, are springing up from day to day.

Truth never suffers by fair investigation; nor can truths be opposed to each other. The sublime and holy doctrines of the Bible never can sustain any injury from the progress of light and knowledge in physical science, however some tenets deemed important by particular sects may seem to suffer. The moral and physical world are in perfect harmony with each other. There is no collision, discrepancy or discord. The phe-

nomena of nature when properly understood elucidate and confirm the doctrines of Revelation. Hence the absurdity of a prevailing prejudice, that the study of the natural sciences, of medicine among the rest, leads to skepticism. Those persons who entertain such an opinion must have but a slender acquaintance with the operations of the human mind. Certainly that progress in intellectual light which brings us acquainted with the admirable arrangement, the exquisite adaptation of means to ends, which the intelligent eye observes in every part of the natural world, cannot be unfriendly to a belief in the great cause which originated and directs the whole. Not only this enlarged view of the harmonies of nature, but many other circumstances conspire to prevent the members of the medical profession from sinking into infidelity. If there be a class of men in society forced, more than the rest, to see, that *man that is born of woman is of few days and full of trouble*, it must be those whose function it is to relieve the distresses of the human family. If any are led to contemplate the futility and transitoriness of human things, it is those whose knowledge of the frail structure of his body has taught them that "*man is but as grass, and the son of man as the flower of grass.*" If, finally, there be any who, beyond others, feel deeply and constantly the necessity of a belief in a future state of rewards, and of the encouragements arising from it, it must be those, whose calling brings them to the bed-side of the expiring man, and who see the trials of his spirit, when his eyes are about to close upon all that is hope-inspiring in this state of existence.

A little philosophy may make men skeptics, but deeper knowledge brings them back to belief again. So pursuaded am I of the truth of this, that I should not fear to predict, that the age of skepticism among physicians is past. The palm of knowledge has been assigned by Dr. Parr, the most eminent scholar of his age, to the Medical Profession, and it appears to me impossible, that those whose acquaintance with nature is so extensive, and who must see a future state of existence as the last glorious link in the chain which extends throughout the universe can be disbelievers.

The subject of the origin of the different varieties of man has been fully treated of in Blumenbach's work *de Gen. Hum. Var. Nat.* He attributes the diversity of colors to the action of the sun. Dr. Samuel Stanhope Smith, the learned President of N. Jersey College, adopted the theory of Blumenbach, and in his "Essay on the causes of the variety of complexion and figure in the human species," has defended it with much zeal, learning and ingenuity. Camper maintains the same opinion. Dr. Pritchard has taken up the question, and argues it elaborately through two large volumes, which contain many interesting facts, and judicious remarks. Like Blumenbach, Smith and Camper, he believes that all the varieties of man had a unity of origin, and supposes that the first departure from the original color might have been a sort of *lusus naturæ*, which, by being favored by intermarriage, became at last confirmed. Mr. Lawrence, following him pretty much, considers that the differences in the several varieties, in physical organization and moral and intellectual qualities, are analogous to the same changes which occur in our domestic animals, and are to be accounted for upon the same principles. These peculiarities being congenital are transmitted, he supposes, from parent to offspring in hereditary succession. A state of domestication is the most powerful in producing these varieties. Although the constitution of man is much affected by external and adventitious causes, such as climate, food, modes of life, &c., this is confined to the individual, and not transmitted to the offspring. He believes, that like the horse, sheep, dog, &c. which present similar diversities, all the varieties of the human race belong to a single species.

Two facts only have been brought forward by Mr. Lawrence illustrative of his theory. Among the African and copper-coloured races individuals are occasionally born, who are *entirely white*, owing to the circumstance of their having none of the colouring principle common to the skin. Among the English and Spanish these have received the appellation of Albinos.

"That singular description of human beings called Albinos, possess a skin of a peculiar reddish, or an unnatural white tint,

with corresponding yellowish white or milk white hair, and red, or at least, very light blue or grey eyes. The cutaneous organ has sometimes a roughness, which has been construed as an approach to a degree of lepra. The hair of all parts of the body, is unnaturally white and soft; it has not the snowy whiteness of old age, nor the elegant light yellow or flaxen appearance of the fair haired (Blondins) German variety: but it is compared to that of milk, or cream, or of a white horse. The eye-brows, eye-lashes, beard, the hair of other parts, and often a soft down covering the whole body, are of the same color. The iris is of a pale rose color, and the pupil intensely red; these parts, in short, are exactly similar to the corresponding ones in white rabbits and ferrets."*

The other fact illustrating the theory of Mr. Lawrence is that of the family of porcupine men, who derived that name from the greater part of their body being covered by hard, dark-coloured excrescences of a horny nature.

"It was not easy," remarks the historian of the case of Edward Lambert, one of this family, "to think of any sort of skin or natural integument that exactly resembled it. Some compared it to the bark of a tree; others thought it looked like a seal skin; others like the skin of an elephant, or the skin about the legs of the Rhinoceros; some took it to be a great wart, or number of warts uniting and overspreading the whole body. The bristly parts which were chiefly about the belly and flanks, looked and rustled like the quills of a hedge-hog, shorn off within an inch of the skin."†

Now these Albinos or porcupine men, it is supposed, if exiled from society, and obliged to take up their abode in some solitary island, by matching with each other would produce a race, at last, differing widely from all the other varieties. "If they had been discovered at some remote period, our philosophers would have explained to us how the soil, air, or climate had produced so strange an organization; or would have demonstrated that they must have sprung from an originally different race; for who would acknowledge such bristly beings

* Lawrence's Lectures. † Philosophical Transactions, No. 424.

for brothers?" Such is Mr. Lawrence's theory—ingenious, but not satisfactory.

A work from the pen of the distinguished professor of the Institutes of Medicine in Transylvania University, has lately been announced, which it is said combats very ably this theory of Pritchard and Lawrence. The work I have not yet had an opportunity of consulting.

Those persons who regard the account of the creation of man given in the book of Genesis as inspired, will of course ask for no farther arguments on the subject. Leaving that history entirely out of view, it appears to me that the supposition of the unity of his origin is attended with fewest difficulties. Nevertheless it must be admitted that we find great embarrassments in the way of accounting for the diversity of complexion and figure upon any known physical principles. Climate, food, and civilization are considered the most potent agents in the production of these varieties. It was remarked by Mr. Hunter that affording domestic animals shelter and nutritive diet, changed their color from a dark to a lighter shade. Regarding the Æthiopian or black complexion as the original type of the human color, it is supposed that as man has passed from a state of barbarity, where he was exposed in summer to burning suns, and during winter to bleak winds, to one of civilization, he has lost his sooty and assumed a fairer hue. But to this view of the subject it may well be objected, that the Egyptians, who gave the first impulse to science and arts, had probably, as we have reason to believe from the industrious researches of Cuvier, the same complexion, as well as the same shaped crania, as the Caucasian in his highest state of culture. Dr. Granville who made a minute examination of a female mummy, found that it corresponded in the shape and size of its head and pelvis, with the female Europeans of the present day.* In fact, every argument seems to me to go to show, that man came originally from the hands of his Creator in a high state of cultivation; and that those nations which are now found sunk in barbarity and ignorance have fallen away from the or-

* Bostock's Elemen. Hist. Phys. vol. 3, p. 239.

iginal stamp. It is the tendency of man to degenerate, not to improve. If he had been created a savage, without the interposition of some higher intelligence, he must have remained one still.

There is no exact correspondence between the blackness of the skin, and the heat of the climate. On the contrary, some travellers have averred, that "the fairest nations, most generally, are those situated near the equator." The inhabitants of Otahiete, not far from the equator, are fairer than the Malays, who inhabit a much colder region. So the Chinese have a lighter hue than the Esquimaux. The tribes of the Papuas of Australasia do not acquire regularly a lighter shade as they recede from the inter-tropical regions; nor do we find any such change occurring among the tribes of North-American Indians, as we travel from the Gulf of Mexico towards the Northern Lakes. They present everywhere the same unchanging, tawny hue. This is very analogous to the lower order of animals in their wild state, who are found uniformly of the same colour.

Among the black population of America, the change from a black to a lighter complexion is not taking place, as has sometimes been fancied, by the action of our more temperate climate. It is believed that where the negro contains no European blood, his color is as dark as that of his remote progenitor, who was brought from the burning sands of Africa a century ago. Certainly a great change is occurring, but it may invariably be traced to another source, than the action of a cold atmosphere. We must however come to the conclusion expressed by Dr. Pritchard, "that the great laws of the animal economy are the same in their operation on all," and that they bear the distinctive marks of belonging all to the same great species. In favor of the influence of society and modes of life in producing these diversities it may be remarked, that in Hindostan, where, owing to the establishment of *Castes*, the same condition and occupation are transmitted from father to son, there are remarkable differences in the aspect of classes living in the same neighbourhood. The artisans, who are sheltered from the sun, are of a lighter complexion than the agriculturists.

To the theory, that the change is brought about by the continued action of the sun's rays upon the skin, there are many, and in fact it would seem to me, almost insuperable objections. The European who goes from a northern latitude to live about the equator, acquires, it is true, a darker complexion, and if the climate affect his health, he may become bilious and sallow. But his offspring are as fair as the relatives whom he left in the regions of frost and bracing winds, or as he himself carried to his new home. Nor does this change take place in the rete mucosum, where the coloring matter of the African resides, but in the cuticle. It does not increase from generation to generation, nor ever acquire a deeper than a sallow shade.

But however produced, the most superficial observer is struck with the diversity of aspects under which man appears. In different climates he is found of every shade of complexion, shape of feature, stature, and grade of intellect, from the delicate tints and symmetrical head of the European, to the African with black skin and depressed cranium; from the Patagonian, rising in height above six feet, to the diminutive Esquimaux; and from the Caucasian skilled in every art, and learned in every science, to the savage Boscheseman, too ignorant to construct habitations, or unite in societies.

It is known that disease is modified by age, sex, temperament, and idiosyncracy. Not only are particular constitutions, under these diverse modifications, more liable to particular diseases, but the diseases themselves must be treated differently. Infants, persons in the decline of life, and those who are in the prime of manhood, though affected with the same complaint, cannot be treated exactly alike. It is necessary to vary the plan according as the patient is of the sanguineous or melancholic, the phlegmatic or the bilious temperament. It is necessary to have the same reference to the nativity of our patients. Those who were born and reared in a northern climate would not perhaps be relieved by a plan which would prove restorative to those of a more southern latitude. May we not suppose, that disease is also modified by the peculiarities which attach to the different varieties of man? From my

limited observation I am inclined to believe that such is the fact; whilst, at the same time, I admit that I am not sustained by the uniform consent of the medical profession, and that a more enlarged experience is necessary to settle it upon a satisfactory basis. The following qualities seem to me to distinguish the Æthiopian from the European variety.

1. They bear heat better, but under the same circumstances they suffer more from cold. Whether correct or not, this is the opinion which generally prevails where the two classes reside together. It is said that they perspire more copiously, which probably accounts for the facility with which they endure a high temperature; and it is possible there is less vigour in their nervous and sanguineous functions, in which case their power of preserving their natural heat would be less. This, however, it is admitted, is nothing more than hypothesis. The theory of animal heat is not yet satisfactorily established. Physiologists have not agreed as to the relative agency of respiration and the nervous system, in its production. The chemical theory, though ingenious and plausible, is surrounded by many difficulties. Till this process is better understood, it is useless to attempt an explanation of the fact referred to. And after all, it is possible, that the only reason why they do not endure cold so well, is, that as a general rule they are worse clad.

2. Left to pursue their natural inclinations they devote a greater portion of their time to sleep; but when kept awake by business, or their amusements, they appear to suffer less inconvenience from the privation.

3. Their skins are thicker, less vascular, as a general rule, and exhale an odour *sui generis*.

4. Sœmmering remarks that, "in the negro the size of the skull bears a smaller proportion to the face and organs of sense than it does in the European; and that the brain is smaller in proportion to the aggregate of the nerves which proceed from it to the organs of sense."

5. The nerves and arteries bear a smaller proportion to the cellular tissue, which seems of undue developement in the

negro race. Hence their being generally of the phlegmatic temperament.

6. They suffer less acutely under disease, if sleeping more, and making less complaint may be deemed evidence. I once saw a white and a black child, about the same age, severely stung with bees. In the first, it produced vomiting, fever, delirium, and a state of nervous excitement bordering upon convulsions; while the latter, although it swelled as much as the other, suffered but slightly. They are not so subject to delirium, nor does the brain and nervous system appear so generally affected. Their diseases are more inclined to fix themselves in the absorbent system.

7. They reach puberty earlier. The females menstruate sooner, and are more universally prolific. I will not say how far these and other peculiarities may be the result of their food, habits, and condition in life. Mulattoes are said to be less prolific than individuals of either variety of unmixed blood. Still they *are* prolific, and this, if the test of Mr. Hunter be received as correct, that when animals of different species copulate, their offspring are unproductive, will establish the fact of the *unity of the human race.*

8. In intellect they are inferior. This would be so, after all that could be done by the discipline of education. Nevertheless, they are highly susceptible of mental culture, and under its genial influence present an aspect happily changed. It has been remarked in all ages, that the crania of idiots differ from those of persons who have sound, vigorous minds; and the heads of negroes depart less widely from their deformity. Daubenton long since observed, "that the position of the head on the spinal column differs in man from that in all other animals, and that the peculiarity exists in a less degree in the negro." The brain is confessedly the organ of intellect, and being less perfectly constructed, the manifestation must be correspondingly defective. They are constitutionally less sensitive and restless, less prone to activity and enterprise, a more contented and happy race.

9. They are more subject to some diseases, and less so to

others. With all their exposure to the damp air and heavy dews of our autumnal nights and mornings, fewer of them are the subjects of fever. The lines of Montgomery, therefore, whatever value may be attached to their moral, are philosophically true:

> Nor in the majesty of storms alone,
> The Eternal makes his fierce displeasure known;
> At his command, the pestilence abhorred
> Spares the poor slave, and smites the haughty lord.

Their attacks of fever occur most usually in winter, and are generally of the typhoid type; owing probably to the debilitating action of cold, which predisposes to disease. The infants are less liable to *cholera infantum*, and when attacked suffer less with it, and more generally recover.

Dyspepsia is their strange disease. I know not that I ever saw a negro laboring under a pure form of this complaint. According to the disciples of Johnson, who view dyspepsia as the effect, merely, of excessive repletion, this fact will be accounted for, upon the principle, that their fare is more uniform, simple and digestible; whilst the converts to the doctrine of Cooke and MacCulloch, who assign to this complaint a miasmatic origin, will attribute it to their unsusceptibility to the action of this poison. From all that I have witnessed of dyspepsia, I am inclined to believe that it is very often a disease of malaria. It appears in situations most subject to febrile disorders, and is often cured by a removal to a pure atmosphere. Like these complaints, it has its exacerbations and remissions, and, like intermittent fever, seasons when the patient feels well. It does not correspond with the quantity or quality of the food taken, very often; and not seldom makes its attack when the person is observing the strictest rules of abstemiousness. Finally, it is often cured by the same remedies that relieve intermittent fever, and the same exposure frequently brings it back.

They are rarely the subjects of mania; but more are idiots. I never witnessed a case of mania in a negro of unmixed blood, though I have seen a number among free mulattoes. They

are also less liable to pulmonary consumption. Although in a majority of post mortem examinations, where the patients died of scrofula, tubercles have been found in the lungs; still during their illness they did not exhibit the symptoms which mark tubercular phthisis in the white subject. Other organs appeared to have borne the onus of the disease.

They are more subject to tetanus. This disease is known to prove fatal to great numbers of them in the West India islands. Every case that has occurred in my practice has been among this race. Trismus nascentium destroys many children, though they sometimes recover under the most unpromising circumstances. During the last summer I saw a girl 10 years of age who labored under the disease from a burn of long standing. She was strikingly of the phlegmatic temperament, possessing less sensibility than is common to a majority of the race. The wound had produced but little inconvenience, and had the appearance of healing. I used opium in its different forms freely, giving upwards of a grain of morphine every four hours, without any perceptible benefit. She was ultimately relieved by tincture of cantharides, a tea-spoonful at a dose, three times daily.

The great scourge of this race is scrofula, which under the vague names of "negro poison," or "negro consumption" carries annually, hundreds to their graves. It is hereditary, and often comes on when it has not been invited by exposure, or poor living. The frequency of the disease, however, results from the inaptitude of climate. The delicate organization of the human frame enables man to adapt himself to every climate, but it renders him, at the same time, more liable to disease. And the condition of the negroes precludes the exercise of that ingenuity, by which the free man is enabled to shield himself against the rigors of the frigid, and the sultry heat of the torrid zone. We find this disease to abound as we travel to the North, and to become less common as we approach the region in which nature cast their lot. They were fitted to inhabit under a different track of the sun, and nature thus shows that her laws may not be infringed with impunity. More ne-

groes, in proportion to the black population, die in Pennsylvania than in South Carolina; and more in Kentucky and Tennessee, than in Alabama or Louisiana.

Cold is regarded by authors as the most powerful of the exciting causes of scrofula. In the East and West Indies says Dr. Gregory, "scrofula is hardly known; but when the natives of either are brought into this, or any other European country, they suffer from it severely." The prevalence of scrofula, he continues, "is directly proportioned to the coldness, or more properly to the variableness of the climate." Lloyd, as quoted by Dr. Good, enumerates "the common debilitating powers of cold, meagre or unwholesome food, want of cleanliness, and a close and suffocating atmosphere," as the most prolific sources of strumous complaints. Dr. Good considers the removal from an intertropical region, where the frame has been debilitated, to countries of a lower temperature, as the most productive cause. The cold he remarks, is not so pernicious when it is constant, and not connected with moisture and impurities, or favored by a scanty or innutritious diet. Hence the peasantry of high latitudes, as among the mountains of Scotland and Switzerland, are much less subjected to it, than the same class in warmer, but crowded cities. "I examined lately," says Dr. Alison, "a register which I know to have been kept with great accuracy for nearly four years, of the deaths of a country parish in Scotland, the population of which is almost exactly 1000 persons. Of 42 deaths that had occurred in that time, 2 only, or 1 in 21, were below the age of 2; and 3 only, or 1 in 14, below that of 5 years; while in the town of Manchester, Dr. Percival assures us, on an average of 20 years, that the proportion of deaths under 2 years to the whole deaths was 1 to 29."* This shows clearly how much diseases, and especially scorbutic complaints, of which a great proportion of these children died, are favored by sedentary habits, and a confined, crowded, impure atmosphere.

It is well known that consumption is a disease of cold climates, while disorders of the abdominal viscera abound most

* Good's Stud. Med. vol. 3, p. 340.

in intertropical regions. M. Flourens, a French savant, having made many experiments on the influence which cold exercises upon animals, has drawn from the conclusion, "that exposure to prolonged cold is the most powerful of the causes which may induce pulmonary consumption; and that, on the contrary, living in a warm place is so powerful a remedy against the disease, that it is of itself sufficient to cure it in all cases where the evil has not reached its highest degree." The same remarks are quite as applicable to scrofula, of which consumption, in fact, is but a modification. The action of cold, therefore, we may say, is the exciting cause of the disease under consideration, and its efficiency, of course, is greatly increased when favored by hereditary predisposition, and deficient clothing.

SYMPTOMS.

The skins of negroes who have the scrofulous taint are uniformly of a pale, dry, and husky or branny appearance. They have, while still able to attend to business, an inanimate and unhealthy look. The tongue and gums are pale, the former often covered with a white mucus. The cornea has a blueish white colour. The alæ nasi and upper lip are generally somewhat protruded. The circulation at first is languid; they bear cold badly; but as the disease progresses, and the tubercles commence their growth, there is some developement of arterial excitement. The pulse becomes quick and irritable, and the heat of the skin is raised. The whole expression of the face, and all the movements of the individual evidence deep seated disease. In the progress of the complaint respiration is disturbed; the breathing becomes quick and laborious, and on slight exertion the patient has the appearance of panting. The lungs seem confined, as if wanting room to dilate. The patient often groans in his sleep. The abdomen becomes tumid; occasionally, towards the termination of the disorder, fluctuation is perceived. The enlargement at first, is from the growth of the liver or spleen, and the deposition of tubercular matter in the mesenteric gland, and upon the peritoneum. The urine is generally scanty; bowels at first constipated,

but disturbed towards the close of the disease, by intractable diarrhœa. The febrile excitement during the whole progress of the disease is never very high. The inflammation seems of the sub-acute kind. An enlargement of the lymphatic glands of the neck sometimes accompanies these symptoms, which is not unfrequently a favourable symptom, evidencing that the disease is disposed to attack external parts. Cough sometimes attends, but not invariably. The appetite is generally unimpaired, occasionally vitiated. The patients generally complain but little; and there seems nothing in their case to account for the decline in flesh and strength which we see steadily progressing. There is no more remarkable feature in the disease than this.

Cases.—One of the first cases in which I had an opportunity of making an autopsical examination, exhibited the following appearances. Patient æt. about 45, died with most of the symptoms of phthisis pulmonalis, his master supposing him poisoned. The lungs were extensively diseased. One lobe so filled with tubercular matter, as to be impermeable by the atmosphere. The other also much disorganized. Not more than a fourth of the organ could have been concerned in respiration. The tubercles were in every stage, from the miliary, to a state of complete maturation. The mesenteric glands contained tubercles; the liver much enlarged and indurated.

Case II.—A negro boy, æt. about 12, of a feeble habit, and phlegmatic temperament, after labouring under the disease for two years, died Feb. 18th 1827. His appearance was always unhealthy; glands of his neck enlarged from his infancy. The first alarming symptoms in his case showed themselves in the winter of 1825. During the summer his health recruited. He laboured under a dry cough, during his last attack; abdomen enlarged; breath short; pulse quick, frequent, tremulous; appetite good; never complained of acute pain. Abdomen so much enlarged towards the termination of the disease as to create the belief that he laboured under ascites. In examining his body, the knife, so soon as it had divided the abdominal parietes, was buried in the liver, which was found enormously enlarged, as well as greatly indurated, and attached firmly to

the peritoneum. Peritoneum studded with tubercles in a caseous state. Right lobe of the lungs a tubercular mass, in the different stages; left contained miliary tubercles. Spleen healthy. Mesenteric glands loaded with tubercular matter; the viscera of the abdomen agglutinated together. Stomach not affected. Heart softened, flabby; right auricle containing a polypus. Pericardium filled with water.

Case III.—The following cases which I noted in the spring of 1824, all occurred in the same family. A boy and girl, children of the same mother, died of consumption when about seventeen years of age. A little girl æt. 10, sister of the above, had a lateral curvature of the spine, so that in walking, which she did with difficulty, one hip was higher than the other. A boy, æt. 8, had abscesses on the hand and leg, which being opened, discharged scrofulous pus. A child still younger, had its maxillary glands enlarged. All the children of the family, eight in number, seemed to labor under the affection in some part. The mother was robust, and had a healthy appearance. The father was not seen.

Case IV.—A mulatto girl, born of healthy parents, but with a delicate constitution, brought on the complaint by exposure, of which she died æt. 20. Her lungs contained tubercles, with specks of black matter, and calcarious concretions. Liver indurated and enlarged. Considerable quantity of fetid, yellow fluid in the abdomen. Heart softened and flabby. Mesenteric glands indurated. In this case there was more febrile excitement than usual, and a greater amount of suffering. Where bark, arsenic, or any of the preparations of iron were used, whether to prevent chills or invigorate the system, they soon created such excitement as made it necessary to abandon them. She was more than once in a great degree cured by a mercurial course, followed by nitro-muriatic acid; but exposure and imprudence still brought the disease back, and she at length sunk under its reiterated attacks.

Case V.—This was a boy, æt. about 14, who was the last of seventeen that had died in one family in the course of a single year. The master was persuaded that they fell victims to ne-

gro poison, and had them examined by a *root doctor*, who was of the same opinion. This case did not differ in any important particulars from those that have been detailed. The lungs and mesenteric glands were loaded with tubercles. All the individuals that died in this family, labored under substantially the same symptoms.

TREATMENT.

Nothing militates more against the successful treatment of this complaint, than the tardiness of masters in applying for medical aid. Proceeding upon the sordid and ill judged principle of pecuniary saving, they either trust wholly to the unaided efforts of nature, or make application to the physician at the last hour.

Much may be done in the prevention of this disease; more perhaps than in most others; for the causes of few others are so well traced. Too little care is paid generally by masters to the comfort and cleanliness of their slaves. Their houses should be roomy, raised considerably above the ground, and so constructed as to admit of free ventillation. To attain the latter end, it is important that they be not too much crowded. Negroes require to be warmly clad in winter; and attention should be paid, especially to the children, that their clothing be often enough changed. This is important on more accounts than one. Dr. Rush in his inquiry into the influence of physical causes upon the moral faculty, remarks, "that too much cannot be said in favor of *cleanliness*, as a means of promoting virtue." It is known that the prohibition of swine's flesh, and the injunction of frequent ablutions of the limbs and body by the Levitical law, had for their end the promotion of virtue, not less than the prevention of leprosy, and other diseases; and hence the writings of Moses have sometimes been called by military men the best "orderly book" in the world. Sir John Pringle, in his account of Capt. Cook's voyage, has some judicious remarks on this subject. "Cleanliness," he observes, "is conducive to health, but it is not so obvious that it also tends to good order, and other virtues. Such, (speaking of the ship's crew) as were made more cleanly, became more sober, more

orderly, and more attentive to duty." This will be the result of the observation of all who will take the trouble to look around upon society, and see how generally vice is associated with ignorance, filth and rags. The elegant Mr. Addison used to declare that he never felt in a mood for writing when not genteelly dressed. I can imagine few things which would contribute more to the health and comfort of slaves, than an increased attention on the part of their owners to cleanliness in their dress and habitations.

Another object of the highest importance is to keep them warm. Many white persons find it indispensable to their health to wear flannel during the winter, which alone affords them adequate protection against the sudden transitions of our variable climate. The same should be done by feeble negroes, and especially those predisposed to scrofula, who certainly are at least as much affected by the dampness and coldness of our winters as their masters. By this simple measure I have seen many beings, who appeared to be in the incipient stage of the disease, invigorated and made to enjoy good health. The diet of our black population, as a general rule, is simple, nutritive and abundant; they inhale our fresh atmosphere without stint or hindrance; and with a little more attention to their clothing—the addition of a flannel shirt to their winter-suit—I doubt not that the lives of numbers who now die yearly of the dreaded "negro poison," might be preserved. Children predisposed to the complaint have frequently a depraved appetite, causing them to eat dirt, and the parents generally from a mistaken view of the cause of the practice attempt to correct it by the application of the rod. This propensity is easily destroyed, and the general tone of the system at the same time improved by the preparations of iron.

Specifics were at one time much relied upon for the cure of scrofula. One form of the disease it is known, took its name from the supposed efficacy of the royal touch. In some parts of this country the root of the poke-weed (phytolacca decandra) has much celebrity for its virtues, and cases have been related to me by men of veracity which would authorize it

trial in some forms of the complaint. But the profession have generally abandoned the search after specifics, and now treat it like other diseases according to its symptoms. The iodine, however, is still used specifically in scrofula, without any very well defined notions of its modus operandi.

Emetics claim a high rank in the treatment of this disorder, and should be administered every day, or every two or three days; by which course we often succeed in arresting the subacute inflammation, upon which the developement of the tubercles probably depends. This plan should be adopted as soon as we have reason, from the febrile excitement and other symptoms, to believe, that tubercles are beginning to form, and must be persevered in for a considerable length of time. Ipecacuan and sulphate of zinc are preferable to tartar emetic, as they create the same nausea, and give the same salutary shock to the system, and at the same time are more transient in their debilitating influence. They may, in fact, be continued for a long time, often with the effect of improving the strength of the patient.

Cathartics are also a remedy of great value in the forming stage of this complaint. The bowels at first are torpid, and during the whole progress of the disease secrete an undue quantity of mucus, on both of which accounts, purgatives are necessary. Contrary to the opinion which has been long and generally entertained, the physicians of this country, at least a majority of them, regard the mercurial as our best cathartics. They give calomel either alone, or in combination with aloes and rhubarb. In some instances it seems contra-indicated, producing great irritability of the system, weakness, &c., in which case some of the vegetable cathartics are substituted. As a general rule I have found no combination of cathartic medicines to answer my expectations better, than the pills of calomel, rhubarb and aloes, so highly recommended by Professor Cooke, which are now in general use throughout the western country. It were needless, in the present age of medicine, to enlarge upon the efficacy of purgative remedies where there is congestion, or a morbid, or diminished secretion of any of

the viscera of the abdomen. In dyspepsia I have derived from them the most unequivocal advantage, especially in the form of laxatives. It is probable that congestion of the abdominal viscera resulting from the action of cold, favors, if it does not originate, the growth of the tubercles, which seem to constitute the essential feature of scrofula. Upon what remedies can we so confidently rely for the removal of this congestion, as upon emetics and cathartics?

Antimonials, or nauseants, are also a valuable class of remedies. Continued for a length of time, they have been known to discuss scrofulous tumors situated externally. They aid very powerfully the action of emetics and cathartics, in subduing that state of inflammation which constitutes the first link in the chain of the disease.

Minute doses of mercury have exercised a happy influence over the scorbutic diathesis in the hands of the judicious and discriminating; but much judgment and caution are required in their use, since constitutions exist in which they could not fail to do much mischief. Perhaps the best form of this mineral is the deuto-chloride, 10 grs. to ℥ij spirit. rectificat., of which the dose is two tea-spoonfuls three times daily. If symptoms of ptyalism occur, the remedy must be omitted.

In countries where a meagre or innutritious diet is one of the most ordinary causes of scrofula, a plan of treatment so antiphlogistic as the one which has been recommended would be deemed unnecessary, perhaps positively injurious. But among the people who inhabit our fruitful valley, where the diet used is so stimulating, where disease is so generally inflammatory, and so often modified by the prevalence of malaria, our constant resort, in nearly all our complaints, must be to depleting and alterative remedies.

If by means of emetics, cathartics, &c. we have succeeded in arresting the disease, our next object is to remove the scorbutic diathesis, and invigorate the system. In effecting this the mineral acids, and especially the nitric, or which is perhaps superior, the nitro-muriatic, are among the best remedies. Their impression is gentle, and at the same time permanent. Sulphate

of quinine is a valuable tonic in such cases. The different forms of iron should be preferred for children, when it is our wish to ward off the disease; and they have great efficacy also in building up the exhausted energies of the frame, when all inflammatory action is subdued.

Such has been the method of cure pursued, occasionally with entire success, by the practitioners of this country, until within a few years. But many cases still occur which obstinately resisting these remedies have made a further increase of our resources desirable. For the last few years the iodine has been introduced into practice. In some cases, as was to be expected in a disease of such a character, it has failed; but its success in others has been such as to awaken lively hopes, that it will yet prove a most valuable accession to our stock of remedial agents. From my own limited experience in the use of it I feel persuaded, that it possesses decided efficacy in removing some of the forms of scrofulous disease. I have had cases where it has been manifestly of advantage; and I know not that I have ever been disappointed when I used it under circumstances where it promised well. That it should always cure cannot be expected. Even bark often fails of curing intermittent fever. It is an interesting fact connected with iodine, that the Saratoga waters, which have been famed for the cure of consumption and scrofula, have been found upon a chemical analysis to contain it; and to it they probably owe their efficacy.

The first case in which I employed iodine was one of strumous ophthalmia in a boy about 12 years of age. He had been affected with the disease for more than a year; and by the direction of another physician had made a fair trial of poke-root. He took by my advice a few doses of calomel, and afterwards commenced the iodine, 6 drops of the tincture at a dose, three times in the day, increasing the dose gradually to 12 drops. He was directed to use at the same time a weak solution of muriate of mercury as a collyrium. At the time he commenced this course one eye was entirely closed, and the other so much inflamed that he could not admit the light except

during the morning and evening. The cervical and sub-maxillary glands were greatly enlarged and inflamed. He had not pursued this plan long before the inflammation of his eyes so far abated as to admit of his walking about: but here they remained stationary. It seemed for a great while that his case neither improved nor grew worse. The course was, however, persevered in, and he has now the appearance of being well.

The second case was of an analagous character in a girl of scrofulous family. In this, application was made early. Several years previously she had suffered seriously from an attack of the disease, in which the sight of one eye was lost. When she applied for advice there was general inflammation of the eye, with slight protrusion of the cornea and enlargement of the cervical glands. She was directed tinct. iodine, 6 drops thrice daily, to be increased gradually to twelve. The remedy acted promptly—the inflammation of the eye, swelling of glands, &c., quickly subsided, and in a few weeks she was entirely cured. There has been no recurrence of the disease since.

I will not detain the reader longer by the detail of other cases differing in no important particulars from the foregoing. Iodine is an article which is worthy of a more extensive trial than it has yet had in such diseases; and in the hands of the skilful will be made, I doubt not, the means of relieving many obstinate and afflicting cases.

Art. VI.—*Remarks on the theories of Clutterbuck and Broussais.* By John Esten Cooke M. D. &c.

[Read before the Medical Society of Lexington, and published by their request.]

It is a doctrine that cannot be controverted with any prospect of success, that the causes of fever, commonly called its remote causes, do not produce that disease by an immediate or direct action upon all, or most of the parts affected. These causes are so dissimilar in their nature, and yet co-operate so evidently, in the production of the same affections of several

different parts, in fever, at the same time that it is evident *no one* of them can have direct access to all the parts affected, that it is impossible they can produce the effects observed by a direct operation upon them. This, however, is not now a matter of controversy. It is, therefore, sufficient to have made the observation, and to draw the inference, which is likewise unanimously assented to, that the causes commonly called remote, in the production of that combination of symptoms which are included in the idea of fever, operate upon most parts of the system through the medium of some one part: that is, that the operation of each cause is upon some one part, (or possibly more than one) and that the effect thus produced on that part, is the cause of the affection of the other parts observed to be disordered. So far, all agree. But the important questions, *What part* is first affected in fever? *How* is it affected? and *How* does that affection produce the other effects observed to follow?—are subjects of ardent debate. Two opinions have chiefly occupied the attention of physicians from the decline of their confidence in the theories of Cullen, Brown, and the European continental writers. It is intended in the present paper to make some remarks upon these. They are commonly known as the theories of Clutterbuck and of Broussais.

The theory of Clutterbuck preceded that of Broussais, having been published in the commencement of the present century.

According to this theory, the part first affected in fevers, commonly called idiopathic fevers, is the brain; the affection of that part is inflammation; and that disturbed state of the brain is the cause of the disturbed state of the various functions of the body in fever, through the influence that that organ is known to exert over the whole system. To this theory the following objections are made.

1. Most of the causes of fever have no access to the brain, and cannot act upon it directly.

The remote causes which Dr. Clutterbuck acknowledges to be productive of fever, are "heat, cold, and especially the alternations of these; intemperance of various kinds; irritation

of different parts, as of the stomach from improper food, particularly food of a certain kind, as crabs and other shellfish, which readily excite fever in some constitutions; irritation of the intestinal canal, by worms, especially in children; certain passions of the mind; putrid vapours; marsh miasmata; human and animal contagion." Of these eleven classes of causes (excepting only the passions) not one can be supposed to have any direct influence on the brain; not one has direct access to it. They all act in the first place, on some other part. Heat and cold, upon the external surface and upon the lungs; intemperance and improper food, upon the stomach and bowels; marsh miasmata and human volatile contagion have access only to the external parts and to the lungs, and cannot therefore act upon the brain directly; fixed human contagion and animal poisons, only act upon broken surfaces, as on the little wound in inoculation, or on that made by a bite, and therefore cannot act upon the brain, directly. Worms and putrid vapours, by which last is here understood those vapours which are offensive and nauseating, if ever they produce fever, constitute no exception to the force of this objection. They have no immediate access to the brain. That one of these enumerated causes which produces by far more cases of fever than all the rest put together, viz. miasmata, manifestly has no immediate access to the brain, and cannot possibly produce any direct effect upon it.

Dr. Clutterbuck mentions a case in which carbonic acid gas produced fever. A boy went into a cellar where a quantity of charcoal was burning. He was seized at the moment of entrance with severe headach, and sickened immediately with a fever of the typhoid form, which continued fourteen days. In this case also it is manifest that the cause had no direct access to the brain, and could not have produced any direct effect upon it.

I have known a blow with the fist, which knocked a man down, produce a dangerous fever which continued above a week.

2. It is objected likewise to this theory, that inflammation

does not exist in very many, perhaps a very great majority of cases of fever.

This is manifest from the total absence of all disturbance in the functions of the brain in a multitude of instances of fever. It is manifest from the known absence of the slightest trace of inflammation of the brain in many cases of fever, even when that disease terminates in death; while even in those cases in which inflammation has been discovered, the symptoms appeared at a late stage of the disease, being clearly a part of the effects, and not the cause of the disease. It is manifest, also, from the fact that even in those cases of fever which are accompanied by the severest headach, there is generally no inflammation of the brain.

This is evident from several considerations. Dr. Clutterbuck speaks of the headach produced by a fit of intoxication in the following manner. "Of this kind" (namely, of causes which are known to exert their action chiefly on the brain and nervous system,) "is intoxication, which is often succeeded on the following day by headach, increased heat and other symptoms, scarcely, if at all, distinguishable from fever generated from other sources." This headach is no evidence of inflammation of the brain: 1, because it goes off with a little rest; 2, because a few cups of good strong coffee will carry it off at once, in many instances; and 3, because if that fail, a little of the same which caused it will remove it almost certainly. If it were inflammation this remedy would increase it.

Dr. Clutterbuck mentions the case of a little girl who whirled herself round swiftly, in play, until she became vertiginous, vomited, and complained of pain in the head, to which succeeded increased heat and thirst: these symptoms continued twenty-four hours and terminated in a critical sweat. Would inflammation of the brain have ceased thus easily?

Dr. Clutterbuck also mentions a case from Van Swieten of a girl who, when in health, being terrified at the unexpected sight of a dormouse, fell immediately into a quartan ague, which continued to recur for a whole winter, and was brought back again after it had ceased, by a repetition of the same cause.

Can it be supposed that inflammation of the brain was in this case *instantaneously* produced, so as *so immediately* to cause the disturbance of the functions observed in this fever? But if this were granted, another objection presents itself. The disturbance of the functions went off with the paroxysm, and the patient had no return of it until the fourth day. What became of the inflammation of the brain in the intermediate time? What became of it in all the long intervals of a quartan ague, continuing through a whole winter? What becomes of it in all such cases, when the patient feels perfectly well in the interval, and eats enough to kill outright any one with an inflammation of so vital a part? Manifestly it does not exist. If its presence be judged of by any known symptoms, it does not exist in the intermissions, and consequently not in the paroxysms following the intermissions. And if not in the paroxysms after the first, there is no evidence of its existence in the first; because the symptoms observed in the subsequent paroxysms, are the same with those observed in the first; and if these symptoms do not show the existence of inflammation in the former, they do not in the latter.

3. Very strong objections to this theory rest upon the treatment of fever.

In the first place, the doctrine seems to agree very well with the advantage derived from bleeding and purging in fever. But it is to be observed that bleeding is positively injurious in many epidemic fevers in which we observe the symptoms supposed by Dr. Clutterbuck to indicate inflammation of the brain, very strongly marked. And in many cases there is the strongest evidence, that wine was beneficial in moderating the disease, and in lessening the frequency of the pulse. Moreover, if inflammation of the brain really be the first effect of the remote causes, and the cause of all the symptoms observed, it must exist from the commencement; and therefore, bleeding ought to be serviceable the moment we perceive the patient to be indisposed; that is to say, in the very forming stage of the disease, even when the pulse is weakest. Bleeding, however, before the pulse becomes strong, will often be found injurious, sometimes fatal.

Again, if the symptoms of fever be produced by inflammation of the brain, bark, wine, &c. would be destructive to the patient in most cases, and in all would be sure to increase the symptoms manifestly. But in cases without number of intermittent fever, (which is the very same disease with the remittent, as to cause, and operation of the remote causes, and parts affected,) bark given in the midst of symptoms indicating the greatest disorder of the brain, and likewise wine, brandy, and opium, have calmed the patient and put an end to the disease.

Again, fevers have been often cured by great excitement of the mind. Thus, Dr. Clutterbuck mentions from Pliny, that the Roman Consul Fabius Maximus was cured of a quartan ague instantly on entering into battle with the Allobroges and Averni. If there had been inflammation of the brain in this case, can any man suppose that the excitement, the violent efforts, the fury of battle would have cured it?

It is evident from these considerations that this theory is unfounded, because the remote causes in almost every case have no access to the part which is alleged to be the seat of the primary affection; 2, because there is evidence in very many cases that the alleged affection of that part does not exist; and 3, because the remedies which are known to be well adapted to the treatment of the disease, in many instances, would be highly improper and even destructive in such a state of the brain; which therefore does not exist.

Let us now turn our attention to the theory of Broussais. That writer maintains that the part first affected in fever is the stomach and bowels; that the effect produced there by the causes of fever is inflammation; and that this inflammation exercises an influence over other organs, producing in them the various symptoms which constitute fever. This theory is exposed to the following objections.

The remote causes of fever have, for the most part, no access to the stomach, and therefore cannot act upon it directly. The remote cause, which produces the greatest number of cases of fever, beyond all comparison, is that volatile agent which arises from the co-operation of heat and moisture on vegetable

matter, commonly called marsh miasmata. This cause is evidently mixed with the air and rests upon the surface of the earth, occupying the low places; because we perceive its effects in such places, and in the direction to which the wind sets from the source from which this agent arises. As this cause, therefore, is diffused in the air, and as we do not swallow air, it is evident that it has no access to the stomach. It may be said, indeed, that a portion of the air is entangled in the food we eat, and that in this manner the agent in question finds access to the stomach. But as it is mixed with a very large proportion of atmospheric air, the small quantity of the latter entangled in the food can afford but a minute portion of the former. It may be alleged, however, that this agent has a great disposition to unite with liquids, and is, in consequence of this, separated from the air and swallowed without it. But if this be the mode in which miasmata find access to the stomach; inasmuch as there is a strong tendency in this gas to unite with water, and consequently every spring and stream of water near where it is generated, must be well imbued with it in the proper season, the gas in the water should be much more operative in the production of fever, than the minute portion that can be swallowed in the act of mastication. We should therefore find the disease most observable in those who drink water most freely in the autumnal season; which is certainly contrary to experience: and on the other hand, those who drink no water, but beer or ale, made in winter, when such an agent is not in the water, ought to be less liable to attacks, which is equally opposed to the truth. We should also find those who are arrested by illness, made much worse by drinking freely of water. But this conclusion, so fairly flowing from the doctrine in question, is altogether contrary to experience. Free draughts of water are decidedly beneficial. Therefore there is nothing in the water injurious to the stomach: but the agent in question is in all the water at that season of the year, and consequently it is not injurious to the stomach. This corresponds with what we know to be true in many other cases. Poisons taken into the stomach are not always injurious. Therefore,

if it could even be proved that this poison has free access to the stomach, it ought next to be proved that it is injurious to it. But so far from any proof of this, there is not the slightest evidence that even the fauces, to which this poison is first applied if it be mixed with the food in mastication, and to which it has constant access, are in the slightest degree inflamed by it.

Again, if this were the mode in which this agent finds access to the stomach, the result of its action on the inhabitants of the opposite sides of a stream of water in the neighbourhood of a large marsh, ought to be very different from that which is observed to take place. In such a case if the wind blow from the marsh across the stream, the miasmata will be carried on to the stream and there be absorbed. If the water thus imbued were productive of fever, as it must necessarily be if the agent applied to the stomach be capable of producing fever, all who use the water of the stream ought to be equally liable to be affected. But this is entirely opposed to the truth of the case. Those on the side of the stream next the marsh, are affected: those on the other escape: and those who on the side next the marsh, are nearest to it, suffer most and very much in proportion to their nearness to the marsh and not to the stream, although the effect ought to be the reverse if the consumption of the water had any thing to do with the production of the disease; because the nearer to the stream, the more free the consumption, and the reverse. It will be recollected that in the seasons of the severest epidemics, the country is often so destitute of water from springs, that that of streams is not only used, but carried many miles for daily consumption. And that the ponds which produce the cause of fever are often so foul that the water is entirely unfit for drink: an instance of this occurred in the famous case of the fever produced near Galliopolis, in 1796, in a portion of the United States' trooops stationed there.

One more reason is worthy of attention. Persons who are exposed to a stream of miasmata, are sometimes affected with fever in so short a time, and when mastication is not going on, or, if going on the substance masticated is of such a nature as

to forbid swallowing, as in the case of chewing tobacco, as to render it manifest that this cause does not get access to the stomach. For these reasons I think it clear that it is not by acting on the stomach that this remote cause produces fever.

With regard to other remote causes of this disease; in the case of fever produced by carbonic acid gas, mentioned already, the first effects were immediately observed. There was therefore not time for this gas to get access to the stomach and through it to influence the other parts observed to be instantly affected.

In the case of the girl affected with fever by whirling round swiftly until vertigo and nausea were produced, it is manifest that the action was not immediately upon the stomach.

In the case of the girl who fell into a quartan ague which continued a whole winter, on being terrified by the unexpected sight of a dormouse, and which was produced a second time by the same cause after it had ceased, it is perfectly obvious that the cause could not have acted directly on the stomach.

In cases of fever produced by a blow on any part of the body, which often occurs, the cause has no more immediate relation to the stomach, and cannot possibly act upon it directly.

Cold is also a remote cause of fever. In many instances autumnal fevers make their attack immediately after exposure to cold; and many authors mention the occurrence of autumnal fevers in cold weather. Vernal intermittents too are evidently brought on by exposure to the violent vicissitudes of heat and cold usual in the spring. Cold is a remote cause so operative, that without it the patient, in many cases, would have escaped altogether an attack of autumnal fever. If therefore the doctrine of Broussais be true, cold also must find access to the stomach and produce inflammation there as the first step towards the production of fever. But this is absurd.

It is manifest, therefore, that a blow, violent excitement of the mind, cold, miasmata, carbonic acid gas cannot act immediately on the stomach. And if we consider what an overwhelming majority of the cases of fever actually occurring, arise from these causes, it is manifest that in most cases of fever the cause

does not find access to the stomach, so as to act upon it in the first place in the production of fever.

It is objected, secondly, to this theory, that the causes are not adapted to the production of inflammation of the stomach, by a primary and direct operation.

How can cold produce inflammation of this viscus, except by immediate application by swallowing ice or very cold water? Then indeed, inflammation may be produced, and it is manifested by the appropriate symptoms. But such action on the stomach is not possible to cold air; nor do such symptoms appear in cases of fever produced by exposure to cold air.

How can passions of the mind produce inflammation of the stomach, by direct operation? Did the fear occasioned by the sudden appearance of the dormouse, in the case already mentioned, inflame the stomach? Can inflammation of the stomach be immediately produced in this manner? Moreover, the disturbance of the functions which, according to this theory, depends upon such inflammation, went off in this case and did not appear again until the fourth day afterwards. What became of the inflammation of the stomach in the intermediate time? What became of it in all the long intervals of that quartan ague which continued a whole winter? What becomes of it in all such cases, when the patient feels perfectly well in the intervals, and eats and drinks enough to destroy him if his stomach were inflamed? Manifestly there is no such thing as such an inflammation in such cases.

How could a blow on the head produce, as in the case already stated, inflammation of the stomach? or how could whirling round swiftly cause such an affection? Who can believe that the boy who fell into fever of fourteen days continuance from going into a cellar full of carbonic acid gas, had inflammation of the stomach thereby produced? Do we not know that a man may swallow with perfect impunity as much of this gas as he can by machinery force water to keep in solution?

It is objected, thirdly, that inflammation of the stomach does not exist in a great majority of the cases of fever.

This is manifest from the perfectly quiet state of the stomach

in many epidemics, and in many cases of every epidemic. The stomach is perfectly free from the symptoms commonly supposed to indicate inflammation of its coats. There is no vomiting even when medicines are administered which are well calculated to irritate it. Surprising doses of jalap, aloes, scammony, and even spirit of turpentine, are retained; which could not happen if the stomach were inflamed. I once had an opportunity of examining the body of a man who died of autumnal fever, under the care of two physicians, after a week's illness, during which they made great efforts to open the bowels without success. The patient had retained without difficulty, large doses of scammony, jalap, calomel, &c. administered throughout the illness, but had had no passage from the bowels. In his stomach was found a large quantity of the medicines in a state about as fluid as that in which such are commonly given; and the coats of the stomach were absolutely free from inflammation; so that it was remarked how perfectly healthy its appearance was.

In many cases, moreover, in which the stomach is not quiet, but disposed to reject its contents, the acrid medicines above mentioned, and wine, spirit, &c. are often retained.

Moreover, when the stomach and bowels are really inflamed, there are no such symptoms as the brown or black tongue, subsultus tendinum, and black discharges from the bowels, common in cases of autumnal fever.

There is one consideration which deserves particular attention. As the agent which produces the great majority of the cases of autumnal fever, has access to every person in the neighbourhood in which it is produced, its effects should appear in a greater or less degree in every body. If therefore the stomach be affected with inflammation in consequence of the swallowing of a portion of the gas in mastication, this state of the stomach should be universal. But of this there is not the slightest evidence; and it is so far from being true, that the season of epidemic sickness is in some persons a time of particular good health. This has been the case with myself for a number of years back, and the reason is obvious. In the

healthy seasons of the year my employments have been sedentary, to excess; while the autumnal seasons have been spent in active exercise on horseback. The consequence of the former has been considerable chronic disorder, often want of appetite, and sometimes acidity and eructations; the effect of the latter improved health and fine appetite, with the use of meat three or four times a day, and this when most of the time was spent, often in night rides, in places in which the cause of the epidemic was most active. It is evident that in this case the cause did not disorder the stomach.

In reply to this it may perhaps be said, that by the same rule every body ought to be affected by this agent in whatever manner it acts. This is certainly true; and this severe test of its truth, the doctrine advocated by the writer not only endures without injury; but it is the very test that it appeals to as the foundation on which it rests. This doctrine is, that the mode in which this agent, miasmata, affects the system, is by interfering with the due action of the atmospheric air, with which it is mixed, upon the blood in the lungs; and by preventing the atmospheric air from fully producing its usual effect upon the blood, viz. the change of its colour from black to red. To this part of the system this remote cause obviously has the most perfect access, and in this way, as far as my observation goes, every body is acted upon: every body has the blood more or less darker than usual when miasmata are mixed with the air we breathe.

If it be asked, why then are not all sick? it is answered, that although the first impression of the cause, according to the doctrine of Broussais, is an actual disease of the solids, which, if it exist, must show itself by its known morbid symptoms; this is not the case with the first impression or effect produced in the other case. The darker colour of the blood only tends to render it less stimulant, and consequently to produce diminished action of the heart, and its inevitable result an accumulation of blood in the interior veins, or congestion. Now these effects may exist in considerable degree without any marked morbid symptoms being produced: and what is more, congestion often

produces effects which are highly salutary, as increased secretion and discharge of bile, which tend directly to keep the usual symptoms of the epidemic from appearing. Although, therefore, every man is operated upon by the agent which produces autumnal fever, and every man has more or less congestion resulting from it, this state of the system may not only exist without producing an attack of fever, but may and very often does produce effects, which, by a wise provision of our maker, themselves directly tend to prevent fever.

The objections arising out of the practice in cases of fever, are as strong as any that have been mentioned.

Bleeding in inflammation of the stomach and bowels is always beneficial. Even when the pulse is weak, it is often practised; and seldom fails to give relief. This remedy however is utterly inadmissible in many cases of fever. If inflammation of the stomach were really the first effect of the remote causes, and the cause of all the symptoms observed, it must exist from the commencement; and therefore bleeding ought to be serviceable the moment we perceive the patient to be indisposed; viz. in the forming stage of the disease, when the pulse is weak.

If such inflammation were present in that stage of the disease, bark, wine, &c. would often destroy the patient, and would always be sure greatly to increase the evil. But in numberless instances these remedies have been given with manifest advantage in the commencement. Of this Dr. Lucas has given abundant evidence in his account of the epidemic of Brunswick County Virginia, in 1821.* Something very similar occurred in the epidemics in other parts of Virginia about the same time.

The same remedies have also been often given in subsequent periods of intermittent fevers, which, as to cause, the operation of the causes, and the parts affected, are the same disease as remittents. And it will not do to question the evidence, that in numberless instances of remittent fever, with what are called typhus symptoms and low pulse, bark and wine, and bark and snake-root tea, have been given with decided advantage.

In the case of Fabius Maximus, above mentioned, was the

* See Med. Recorder for July, 1822.

excitement produced by entering into battle, calculated to cure inflammation of the stomach? I have known a man to be cured of a quartan ague, which had long troubled him, by riding forty miles on a hard-trotting horse on the day it was to have returned. Was this remedy adapted to cure an inflammation of the stomach?

If inflammation of the stomach were actually present in the forming stage of fever, vomiting by tartarized antimony would greatly injure the patient. And so say the advocates of the doctrine in France. But the fact is that this remedy often puts an end to a fever at a blow, and in every stage of the disease is a most powerful means of restoring the patient to health.

If such were the state of the stomach in fever, the acrid cathartics, such as jalap, scammony, aloes, colocynth, &c. would invariably increase the disease. But this is so far from being true, that in the great majority of cases they produce not the least disturbance; but operate mildly in the production of a free secretion and discharge of bilious matter, even in young children; whereby the fever is speedily controlled. And if there be a difficulty in procuring an operation, these medicines often lie in the stomach without producing any disturbance, until by perseverance the physician succeeds in his object. If however the doctrine of Broussais were correct, such medicines ought continually to be rejected by the stomach. But it is a very rare case indeed, in which we cannot administer enough to operate well on the bowels. And even when the failure is complete, sometimes the patient continues to the last to take the medicines without once rejecting them, and when dead, as when there is no operation effected in our autumnal fevers he must die, not the least appearance of inflammation of the surface of the stomach is to be perceived.

Even in those cases in which there is great disposition to vomit, showing great inflammation, if inflammation were the cause of the symptoms, and every dose of scammony, aloes, and calomel should aggravate the disease, if the doctrine were true, the stomach not only bears the medicine, but often becomes more quiet even before any increased secretion and dis-

charge of bile is produced. In this state of the stomach I have often found that water or tea, as hot as the patient can possibly swallow it, are useful in allaying the disposition to vomit. When on the first visit to a patient I have been informed that he could retain nothing in the stomach, I have seldom failed to enable him to retain a large dose of calomel, by sitting by him and causing him to drink every few minutes a draught of very hot water.

The vomiting in fever has often been checked, likewise, by frequent draughts of some mild liquid containing a superabundance of that very gas, the carbonic acid, which Clutterbuck mentions as *one of the causes* of this very state of the stomach. That it causes fever is certain, and therefore certain that it causes inflammation of the stomach, if such inflammation be the first effect of the causes in the system. But experience shows that so far from causing, it alleviates greatly the very symptom asserted by Broussais to indicate inflammation of the stomach. The contradiction is palpable. If it cause inflammation of the stomach, as it must according to Broussais if it cause fever, this gas ought to increase the vomiting if that symptom were the result of inflammation.

If, moreover, the doctrine of Broussais were true, all that would be necessary to cure the patient would be to bleed him, blister the epigastrium and keep him quiet, cool, and without any stimulant food or drink; by which means the inflammation would gradually subside. But this is so far from being the result, that even in those cases in which there is no mark of irritated or inflamed stomach, the patients die if a discharge from the liver do not come in to their relief; and live if continually pressed with such cathartics as are above mentioned, provided they act freely on the liver.

When with excessive vomiting there are great discharges of serous fluid, the latter ought to relieve the former if it depended upon inflammation of the stomach. But this is not the case. The patient vomits and purges to death, unless the disease be arrested: and it may almost certainly be arrested by calomel, and very generally by aloes and calomel, or scammony, aloes

and calomel. And these medicines put an end to the vomiting as soon as they produce free bilious discharges; whereas if the vomiting depended upon inflammation, every dose ought to increase it.

Again, if the doctrine of Broussais were true, blistering the epigastrium in the commencement ought to arrest the progress of fever. For we know that blistering removes inflammation of the pleura, and of the intestines, and therefore it ought to remove it from the stomach, and so arrest fever, if inflammation be the first stage of fever. But blistering I have often seen tried and utterly fail. I have seen a blistering plaister applied to the whole epigastrium and abdomen, when the patient was lying complaining of nothing but uneasiness in those parts, without the least alleviation of the symptom. He was at the same time inclined to stupor and had not had a passage from the bowels for many days. His physician had even given up all hope of effecting any. He was advised to give him thirty grains of calomel and as much scammony, in a powder. This was administered without delay and operated six times copiously: the discharges were viscid and of a deep green colour, and the patient was greatly relieved of the uneasiness of his stomach and bowels. The same dose was given on each of the three following days with similar good effect; when he was left to the care of the attending physician and soon recovered perfectly.

Blistering plaisters so commonly fail in removing the irritable state of the stomach, which is sometimes so much in the way in the treatment of autumnal fevers, that I am never satisfied with having a patient teased by such an application: and this is not from any prejudice against this remedy. On the contrary, I have the greatest confidence in the benefit to be derived from it in inflammation of the pleura, &c. and in external inflammations, founded upon long experience of its prompt and decided good effects in such cases.

If inflammation of the stomach and bowels were the commencement of the train of symptoms called fever, and their foundation, the application of leeches to the epigastrium would

immediately arrest the disease. It is, indeed, asserted in a French publication, that if an hundred cases of fever occur in a town and "are all met the first day by leeching the epigastrium, they will be stopped."* But this cannot be true. For if it were, the contest on this subject would long since have been settled; because of the extraordinary success of the practice, and the instant benefit arising from it, in comparison with any other mode of treating fevers. Leeching the epigastrium in fevers, I can say nothing about from my own experience. But if it had been found as successful as it ought to be if the doctrine of Broussais be correct, we should have heard of it before now, from those in this country who advocate that doctrine. In Philadelphia leeching is very common and has long been practised in a great variety of affections. The advocates of Broussais's theory have no doubt tried the application of them to the epigastrium in fever: and if it had been as decidedly beneficial as it ought to be if that theory be well founded, and as Broussais's French disciples assert, we should have had it urged upon us continually as the test of the truth of his doctrine and the sure proof of its correctness.

It is evident, therefore, that the doctrine of Broussais, that the stomach is first affected in fever, and that that affection is inflammation, is not well founded; because the remote causes in almost every case, have no access to the stomach, which is alleged to be the seat of the primary affection: because there is evidence in very many cases that the alleged affection of that part does not exist; and because the remedies which are known to be well adapted to the treatment of the disease, would be highly improper, and even destructive in such a state of the stomach as is alleged to exist.

This being the case, it is manifest that the symptoms of disease observed in the other parts of the system, do not arise from the influence of the stomach over those parts. For, the stomach not being first affected, the other parts cannot be affected by sympathy with it.

There is, indeed, no evidence whatever, of the actual ex-

* Johnson's Medico-Chirurgical Review for July—October 1826.

ercise of influence by the stomach, over other parts of the system. The only evidence produced, is the fact of the simultaneous appearance of the affections of different parts. But this does not prove the influence of the stomach over other parts, any more than it proves the influence of those parts over the stomach. "When the stomach is in a sound state," says Dr. Whytt, "and digestion is properly performed, the spirits are good, and the body is light and easy; but when that organ is out of order, a languor, debility, melancholy, watchfulness or troublesome dreams, the night-mare, &c. are the consequences." It would be just as correct to reverse this, and say; When the spirits are good, and the body is light and easy, the stomach is in a sound state and digestion is properly performed; but when a person feels languor, debility, melancholy, watchfulness, or troublesome dreams, the night mare, &c., the stomach is out of order.

It is, indeed, admitted by high authority* that the term sympathy is the expression of the facts observed respecting this influence. But the facts observed are, that, of the parts, the head, the stomach and the liver, sometimes one and sometimes another is first affected; any one may be affected first, or second, or third in order; and sometimes all are affected together at one and the same time so nearly that it is difficult or impossible to say which was first. Here then, is no better proof of the influence of the stomach over the liver and head, than of the influence of the liver over the stomach and head, or of that of the head over the stomach and liver. There is proof, however, of an influence over them all, exerted so constantly, so uniformly in cases of fever, as to carry conviction that some cause is operating upon them; and as all are affected, and no one always first in order, so as to have any superior claim to such influence, it can be no one operating upon the rest, but some cause operating upon all: and what that is, it is not difficult to determine. Congestion is always present in these cases; it does act upon all the parts named: it is capable of producing the phenomena observed; and therefore it is that cause.

* Blane's Med. Logic, Sect. I. 10. Professor Caldwell's Analysis of Fever, &c.

Suppose for a moment that inflammation of the stomach were capable of exerting an influence over the other parts, and causing the symptoms we observe, the increased secretion of the liver, for example; and we ask how it produces it? If this question cannot be answered, we have nothing but the assertion of the fact of the influence; and have gained nothing: because the mere fact rests, as has been already stated, on observation; and observation shows us no more influence of the stomach over the liver, than of the liver over the stomach. If an answer be attempted, it must be allowed that the influence that produces the increased secretion must act by physical means; either by a direct operation of the stomach on the liver, which is absurd; or by means of a succession of effects, each being the cause of that which follows it. This being the case, we ask, how can there be an increased secretion and flow from the liver, without an increased fulness of the vessels of the liver to supply it? And how can there be increased fulness or congestion of the vessels of the liver, without congestion in the large veins supplying the liver, and in the vena cava with which it is so closely connected? And how can this congestion of the blood in these great veins of the interior, be suddenly produced, as in the early stage of fever, without a corresponding diminution of the quantity of blood circulating in the arteries? And how can this reduction take place without a corresponding diminution of the volume of the arteries? in other words, a smaller pulse; indicating beyond controversy weakened action of the heart. Neither of these, it will not be denied, can exist without the existence of that preceding it; and every one of the phenomena stated, are observed actually to occur in every fever. There never was a fever without diminished action of the heart in the commencement, indicated by diminished pulse, or diminished fulness of the arteries; diminished fulness of the arteries cannot exist without corresponding accumulation of blood or congestion in the vena cava and its branches; and there never was a fever without such congestion in the commencement. If therefore the influence of the stomach produce the increased secretion of the liver, it must be through

this series of changes; weakened action of the heart, and accumulation of blood or congestion in the interior veins: that is to say, it must produce weakened action of the heart, and through it congestion in the interior veins, and increased secretion. But how does it produce weakened action of the heart? This difficulty is not yet solved. We see no connexion between inflammation of the stomach and weakened action of the heart. We should rather expect that inflammation should be connected with increased action of the heart. Let us then follow up still farther the series of changes taking place in the system in the production of fever. We have seen that increased secretion pre-supposes venous congestion; and venous congestion, diminished action of the heart. The latter may flow from the diminution of the stimulant power of the blood, or from diminished power to act. The power to act is not diminished; for increased action takes place in a few hours afterwards. But the stimulant power of the blood is diminished; for in these circumstances the blood is of a darker colour than usual; and experiments made by Goodwynn and many others show unquestionably, that dark coloured blood does not stimulate the heart to as vigorous action as the red blood does. Here is another link in the chain of causes, darker-coloured blood; and if we ask, does inflammation of the stomach cause this? we can see no connexion between the two. But we observe that whenever miasmata abound the blood becomes darker; and between these two, the presence of miasmata in the air and a darker colour of the blood, we perceive a very natural connexion. The atmospheric air by its action on the blood in the vessels of the lungs, through their coats, gives it a red colour. The presence of some other gases than those which compose the atmospheric air, or a diminished proportion of one of these, prevents this change of the colour of the blood from being fully effected. We know, for instance, that one of the causes of fever mentioned by Clutterbuck, as well as by others,* viz. the carbonic acid gas, will, by mixture with the atmospheric air, undoubtedly prevent the blood from acquiring the usual red

* Motherby. Art. Carbo Fossilis. See Parr also.

colour. These causes of fever, therefore, miasmata and carbonic acid gas, are often mixed with the air when fever is produced; they have the most ready access to the lungs, and therefore have the most thorough opportunity of interfering with the action of the atmospheric air on the blood in the lungs, and preventing the change of colour from black to red; this change is observed not to take place when they are present, and under their influence the blood is darker than usual; blood of a darker colour than usual, does not stimulate the heart to action as vigorous as common; weakened action of the heart gives rise to congestion, and congestion to increased secretion, and other effects. Here, then, is a series of causes and effects from two of the remote causes down to the increased secretion of the liver, a prominent symptom in very many cases of autumnal disease; and to every other symptom which is the effect of congestion. There is no room then for the alleged influence of the stomach over the liver. The series of cause and effect is complete without it.

Moreover, the disordered state of the stomach being admitted to be an effect of the remote cause, must be the direct effect of that cause, or the effect of some change produced by it. That it is not the direct effect has been already shown. And if we ask the question of each of the successive changes abovementioned, Is it the effect of this? we shall find that it cannot be the effect of a darker-coloured blood, or of weakened action of the heart; but it may readily be the effect of congestion. And when we reflect, that congestion cannot exist in the veins which supply the liver with blood for the purpose of secretion, without also existing in the veins of the stomach, because the latter empty into the former; that congestion of the veins of the stomach is just as capable of deranging the secretion of the stomach, as congestion of the veins of the liver is the secretion of the liver; that derangement of the secretion of both occurs at the same time, and that when by free evacuations from the liver the congestion of the interior veins is removed, proper secretion returns in both at the same time; it is manifest that congestion is the cause, the presence

of which deranges the secretion of both together, and the removal of which allows both to return at the same time to an ordinary state of secretion.

It is evident, therefore, that the disordered state of the stomach in fever is an effect of the remote cause, produced by the same series of changes that occasions the disordered state of the liver; and that the connexion observed between these two viscera, arises from their both being under the influence of congestion in fever, which deranges the secretion of both at the same time. Consequently the deranged state of the secretion of the liver is not the result of an influence exerted over it by the stomach; or of sympathy with the stomach.

MISCELLANEOUS INTELLIGENCE.

Mode of arresting Hæmorrhage from extraction of the Teeth.—A very severe and dangerous hæmorrhage occasionally follows the extraction of a tooth. That this is, in some cases, the result of a hæmorrhagic tendency in the constitution; in other words, of a want of contractile power in the coats of the arteries, seems clear, from the circumstance that some persons never have a tooth extracted, without considerable hæmorrhage continuing for a long time afterwards. Several cases are on record which have terminated fatally, notwithstanding every effort which was made to stop the flow of blood. The usual mode of treating these cases is to roll up a small bit of lint in a conical form, and to press it into the bleeding alveolar cavity, and then to produce pressure by a compress placed over it. These plugs are often imbued with styptics of various kinds, and caustic is occasionally introduced into the alveolar cavity. Failing these remedies, the actual cautery has been repeatedly used, and even the carotid artery tied without effect.

When it is recollected that the bleeding vessel is situated at the very bottom of the alveolar cavity, it is astonishing that the simple method of applying pressure in immediate contact with it, which I am about to recommend, should not long ago have been adopted. The plugs of lint which are usually employed, are so clumsy and thick that they cannot be forced half way down the socket; and even if they are made more nearly to fit the cavity, the pressure which can be made upon them is very imperfect and uncertain. I have seen many very severe cases, and some in which the patient had very nearly sunk from loss of blood; but I never saw one in which I failed to stop the hæmorrhage in a very few minutes by the following simple process. The bleeding alveolus is first of all to be ascertained, the coagulum carefully removed from it, and the interior cleaned by a piece of lint, and by rinsing the mouth with warm water. A strip of lint of sufficient length being torn off, one extremity of it is then to be introduced by means of any curved instrument which will reach the bottom of the socket, and pressed down to the very extremity of the alveolar cavity; a further portion is then to be forced upon the first, and so on until the cavity is

filled, and every part firmly consolidated, by each portion being pressed upon, by the next above it: when the cavity is thus filled and the lint rises rather above the edge, a compress is laid upon it, so thick as to be pressed upon by the antagonist teeth. The mouth is then to be forcibly closed so as to retain the plug in its place, and keep it constantly and very firmly pressed against the bottom of the alveolar cavity.—*Thomas Bell on the Teeth.*

Case in which a large spoon was swallowed, and produced ulceration of the duodenum and fatal peritonitis, by Mr. Houston. (*From the Dublin Hospital Reports*, Vol. v. page 319.)—The body of a maniac who died with symptoms of *peritonitis* in the Richmond Lunatic Asylum was brought to the College of Surgeons for dissection. The peritonæum exhibited all the marks of violent inflammation, viz. copious effusion of lymph, and seropurulent fluid, with extensive adhesions. A small quantity of the contents of the duodenum was found out of the bowel in the left hypochondre, and a large rusty iron spoon, eleven inches long, was found lying across the spine in the cavity of the bowel with the handle directed foremost and downward, where it had produced ulceration and perforation of the bowel, while the large end was lodged in the *pylorus*, which, though dilated, was neither ulcerated nor broken. The spoon was bent in the centre, where it rested on the vertebræ; but whether it had this form previous to being swallowed, or had acquired it afterwards, it was impossible to ascertain. From the position of the spoon, Mr. Houston infers that it had not been swallowed accidentally in the transmission of food, but had been forced down the œsophagus by the insane patient.—*Edin. Med. and Surg. Jour. Jan.* 1831.

Injury of the neck by Suspension. (*Dublin Hospital Reports, Vol. v. p.* 317.)—From the bodies of two felons executed for murder under the combination system, Mr. Houston gives the following account of the injuries produced by suspension by the neck till death takes place.

"The cervical vertebræ were unbroken, and the spinal marrow and brain presented no marks of injury. In both subjects the sterno-mastoid on the right side (the opposite to that on which the knot of the rope was applied) were ecchymosed, contused, and broken; the sterno-mastoid muscles on the left side were only slightly bruised. The *os hy-*

oides and thyroid cartilage were completely severed from each other. The omo-hyoid, sterno-hyoid, and thyro-hyoid muscles were so bruised and lacerated that only some stretched threads of of them remained holding the parts together. The thyro-hyoid membrane was also torn across, and the epiglottis, pulled from its root at the back of the thyroid cartilage, had passed up with the *os hyoides* and tongue into the back part of the mouth; the skin alone remained unbroken, and interposed between the rope and the cavity of the pharynx. This was the only region of the neck which gave evidence of much injury. All the large vessels and nerves escaped unhurt."

In two other criminals which Mr. Houston had a more recent opportunity of examining, precisely the same effects were produced by the rope on the soft parts of the neck, and without any injury to the brain or spinal chord.—*Ibid.*

On the most efficacious mode of applying sinapisms. (*Archives Generales de Medecine, Septembre* 1830.)—The following observations by *MM. Trosseau and Blanc* will be found useful in settling the proper method of using mustard for sinapisms. They were induced to make some careful experiments on the subject in consequence of the discrepant statements made in works on materia medica. Some recommend that the flour of recently ground mustard should alone be used, others that the bran only of the flour should be taken. Some direct the flour to be made into pulp with warm water, others with vinegar, others with concentrated acetic acid, others with warm water or warm vinegar, indifferently. Some advise that the sinapism be left applied for four hours, others for two or three hours, and others, again, for a single hour only. These authors have very carefully determined the respective advantages of these several practices; and it appears from their experiments, that the several circumstances of difference are very far from being immaterial.

Their experiments were made chiefly with the black mustard commonly used for culinary purposes in Paris. They first found that the flour loses scarcely any of its activity by keeping. In their standard experiment, they remarked, that recently ground mustard, mixed up to a proper consistence with cold water, caused slight smarting in five minutes, and in ten minutes the heat and sense of burning which characterizes the full operation of the sinapism. They next remarked that flour five months old, did not cause smarting within seven min-

utes, but brought on the full sense of burning in ten minutes, or as soon as the sample of fresh flour. They then found, that when the fresh flour was mixed up with hot, instead of cold water, the smarting commenced in three minutes; but the full sense of burning was not developed in less than ten minutes; and, consequently, although hot water accelerates the commencement of its action, the effect is ultimately and in a very short time the same when cold water is used. The most remarkable results, however, were obtained from their experiments with acetic acid. When the sinapism was made with common vinegar, instead of water, its activity was not increased, as is commonly supposed to be the case: it was, on the contrary, much retarded and diminished. In fifteen minutes, there was extremely slight smarting, which increased so slowly, that in fifty minutes, it was not greater than was produced by the water sinapism in six minutes. The substitution of warm for cold vinegar did not increase its activity. Neither was any advantage gained by substituting for common vinegar concentrated acetic acid, diluted with its volume of water; on the contrary, no effect whatever was then manifested in forty-five minutes. Nay, concentrated acetic acid itself appears not more powerful than simple water. A sinapism made with it had no effect in six minutes; in seven minutes, there was slight smarting; and the full sense of burning was not developed till the twelfth minute. These are interesting facts, because they show that the mustard-flour and the acetic acid possess the property of mutually moderating the rubefacient or irritating properties of one another. Concentrated acetic acid itself seems, from the experiments of the authors, to be one of the most rapid, powerful, and convenient rubefacients that can be employed. When applied on a sponge, or mixed with saw-dust, so as to form a mass like that of a sinapism, acute smarting pain was produced in a minute and a-half; in a minute more, the pain was insupportable; in three minutes from its first application, the experimentalist was compelled to remove it; and notwithstanding the short interval which was thus allowed to pass before its removal, the corrosion produced left a mark three months afterwards. A singular fact, which they have not been able to explain, is, that the activity of a sinapism made with English mustard is not lessened, like that of the French mustard, by substituting vinegar for water.

The following is their account of the several consecutive effects of a sinapism made with water, and of the precautions which should be

observed in using it. The first effect is prickling or smarting, which begins in five minutes; in ten minutes, this is converted into acute burning pain, such as is produced by hot iron held near the skin; next, a deeper-seated sensation is felt like that of constriction, or of a heavy weight pressing on the muscles; but after twenty or twenty-five minutes more, the original burning pain returns more violently than ever, and few, who are not insensible from some affection of the brain, can support the sinapism much longer. After its removal, the impression of the cold air relieves the burning for a time; but this, in a few minutes, returns acutely, is soon attended with an uniform, rosy efflorescence, and may continue more or less severely for twelve hours or several days. These authors maintain, that if a sinapism is properly made, and of good materials, it should very rarely remain applied longer than between forty-five minutes and one hour; and that, if it is left for three or four hours, as some direct, its effects may sometimes prove extremely unpleasant. In cases of cerebral oppression during fever, they have known severe sloughing induced by the neglect of this precaution, the physician having been misled by the patient not complaining of pain, and having allowed the sinapism to remain for several hours.—*Ibid.*

Cholera Morbus.—The epidemic disorder, known under this name, has of late years committed the most frightful havoc among various nations in Asia. Within the last fifteen months this awful scourge of human life has made its appearance in several provinces of the Russian Empire with undiminished severity. No medical work hitherto published on this fatal malady has been found satisfactory, nor have the suggestions contained in such works succeeded in arresting its devastating course, which, on the contrary, becomes every day more extensive, and seems to threaten the whole of Europe.

Moved by a deep feeling of humanity, the Imperial Government of Russia deems it right to call the attention of the medical profession in Russia, Germany, Hungary, Italy, England, Sweden, and Denmark to this vitally important subject, and to propose to them to forward to the said government treatises on the cholera morbus, which shall embrace the following points.

1*st*, A clear and detailed account of the nature of the disease.

2*d*, A statement of the causes which give rise to it.

3*d*, A description of its mode of propagation.

4th, A demonstration, by the means of exact and faithful experiments, of its being communicable from one individual to another, if such be the fact.

5th, An indication of the measures to be adopted for self-preservation against its contagion should the disease prove contagious; and,

6th, Of the means best calculated to insure a recovery.

Such Treatises or Essays may be written in the Russian, Latin, German, English, or Italian languages, and directed to the "Conseil de Medecine" (Medical Board) at St. Petersburg, until the 1–13th September 1831. The name of the author is to be forwarded in a separate envelope.

The author of the best treatise, who shall have complied fully with all the above conditions, will be rewarded by the Imperial Government of Russia with a sum of twenty-five thousand roubles in bank paper, amounting to about eleven hundred pounds Sterling, (L. 1100.)—*Official Notice from the Russian Government.—Ibid.*

Observations and Reflections on the Employment of Strychnine in the Treatment of Paralysis. By E. Geddings, M. D. Lecturer on Anatomy and Surgery, Charleston, S. C.—The first case we shall detail is one of hemiplegia of the whole of the left side of the body. The individual was a male, aged about fifty, who was employed as a boat hand. He was placed under our care about the 20th of July, 1829, and from all the particulars of his case, which we were able to collect, it appeared that he had been very suddenly attacked with the disease, about a month previously, whilst engaged in cutting wood. He was seized with violent pain of the head, left arm and leg, fell down in a state of insensibility, to which succeeded, in a short time, a total loss of sensation and motion of the leg and arm, and double vision. He was still in this condition when we first saw him, with the exception that the double vision had disappeared, and he was so far sensible, as to reply rationally when addressed in a loud voice. There was no febrile disturbance, no impediment in the powers of speech, and the principal difficulty consisted in a loss of sensation and motion on one side. The leg and arm could be violently pinched or pricked without the patient evincing the slightest feeling, and the only pain he complained of was in the head and back of the neck.

He was ordered an active cathartic of calomel combined with jalap, principally with a view of creating a revulsive impression from the

head to the lower intestines. Directions were also given to have his head shaved and blistered, and to have sinapisms applied to the legs.

21*st*. The medicine has operated, and the blister has drawn well, but without producing any improvement in the condition of the patient, except a slight amelioration of the pain of the head and neck. He is restless, and his mind somewhat wandering.

22*d*. The same condition. Cathartic directed to be repeated, and cups to be applied to the nape of the neck, the part afterwards to be covered by a blister.

23*d*. No improvement. Ordered sinapisms to the legs, and stimulating frictions to the paralytic members.

24*th*. The same condition continuing without any amelioration, we resolved to put him on the use of the strychnine. The following was therefore directed:—℞. Strychnine, gr. vj.; alcohol, ℥i.; of which six drops were administered morning and evening.

25*th*. The dose of the medicine was increased to ten drops twice a day.

26*th*. Experienced this morning considerable spasmodic action of the muscles of the paralytic members. Mentioned the circumstance without being questioned, and compares the sensation to that which might be occasioned by living animals leaping in the flesh. No affection of any but the paralytic muscles. Complains, for the first time, of pain when pricked with a pin. Medicine to be continued.

27*th*. Slight improvement. Complains of pain in the back of the neck. Ordered a blister to be applied to that part; tincture of strychnine to be increased to fifteen drops.

28*th*. Pain relieved. Can flex the leg slightly without assistance. The sensibility is also improved. Has had twitchings of the muscles of the arm. The paralytic leg and arm have become œdematous. Medicine to be continued.

29*th*. The improvement is progressive. The leg when pricked suddenly was flexed with great force. Arm, however, in nearly the same state.

30*th*. Can flex the leg to a right angle with the thigh, and again extend it. Has had twitchings of the arm and leg. The same prescription continued.

31*st*. Further improvement. Can flex and extend the leg with great freedom. No perceptible improvement of the arm.

August 1*st*.. The improvement is progressive. Medicine to be increased to eighteen drops three times a day.

2*d* and 3*d*. Improvement continues. The muscles of the arm affected still more with spasm. Medicine increased to twenty-four drops.

4*th*. The condition of the leg continues to improve. The arm, however, remains in nearly the same state. From this time the amelioration was uninterrupted, and at the expiration of about a week he began to walk with the assistance of a crutch, and in a short time the power of motion in the leg so far improved as to enable him to walk to any part of the city. He finally recovered sufficiently to be sent into the country. At the time of his departure, however, the arm had not regained its powers, and had undergone but a very slight amelioration. Since then, we have not been able to learn any thing of his situation.

The next case we shall detail is one of a very different character. Thomas Wood, aged twenty-eight years, was admitted into the Charleston Alms-house Infirmary, on the 18th of October, 1829, labouring under general paralysis. Of his precise condition at the period of his admission we are not acquainted, as we were not at that time attached to the institution. One thing, however, is certain, that he was completely helpless, and we believe his general health was very much impaired, from the intemperate use of ardent spirits, and the exposures and privations incident to his habits. About the middle of January, 1830, when he first came under our observation, his general health had improved considerably under the treatment to which he had been submitted, but the paralysis had undergone but little or no amelioration. His legs and arms were so completely powerless as to render him incapable of helping himself, and whatever he took by way of food or medicine, had to be put into his mouth by another person. His legs were drawn up to a right angle with the thighs and could not be straightened. His bowels had, moreover, so far lost all power as to be incapable of expelling their contents, except when inordinately excited by stimulating enemata. The state of the patient could not be attributed to any thing like extravasation within the cranium, as he never had suffered any thing which could lead to a suspicion of such an occurrence. His previous habits, as well as the general aspect of the case, furnished a strong presumption that it originated from the long-continued and intemperate use of bitter beer and ardent spirits, especially gin, which had probably been adulterated

with acetate of lead. That this was the case we are disposed to infer, independently of other considerations, from the fact, that we have met, in hospital practice, and amongst the poor, with a number of cases of paralysis, and have generally found, that the subjects of these attacks have been persons addicted to the immoderate use of inferior gin and beer. These cases have, moreover, all presented the usual manifestations of paralysis from lead.

Taking this view of the case, we resolved to try the effects of strychnine, administered in liberal doses. He was, accordingly directed to have daily a strong aloetic injection for the purpose of unloading the bowels, to be followed, after the operation was over, by half a grain of strychnine, suspended in a small quantity of mucilage. He was also directed to take the twelfth of a grain of the same medicine night and morning. This treatment was instituted on the 15th of January, and was continued until the 24th. On the 16th a splint similar to Pemberton's was applied to the left arm by way of experiment, but after being worn for some time, it appeared to be rather prejudicial than advantageous, and was consequently omitted.

24*th*. The strychnine, in the doses above mentioned, was directed to be given thrice a day. The aloetic injection and the half-grain of strychnine to be continued as before. On the 26th the medicine was increased to the sixth of a grain night and morning. The compound aloetic pills were also directed every night, in sufficient doses to excite the lower bowels. Strychnine injection still continued. No further alteration was made in the treatment until the 8th of February, when the medicine was increased to three pills, of the twelfth of a grain each, night and morning. The aloetic pills and strychnine injection to be continued as above. A carved splint, long enough to extend from the nates to the heel, and also bound upon the posterior part of the thigh and leg, on one side, with a view of overcoming the flexed position of the member. After using it, however, for several days, without any benefit it was abandoned. On the 16th, in addition to the other remedies, we directed the muriated tincture of iron with a view to its tonic effect, and it, with the other remedies, was continued until about the middle of March.

For several days after the strychnine was first administered, very little change was observed in the condition of the patient, and the usual tetanic contractions were scarcely observable; nor were they considerable at any period during the treatment. In the course of a

fortnight a marked improvement was observable. The patient began to flex and extend his arms with facility, and could grasp any thing in his hands with considerable firmness. From this time, his improvement was progressive but slow. His bowels soon began to act with more freedom; his ability to move his limbs increased, and in a short time, he so far recovered the use of his arms, as to turn and move himself in bed, and take his food and drink without assistance. By the 1st of March the mobility of the members had become so far restored, that nothing seemed to prevent him from walking but the flexed position of the legs and a want of confidence in his own powers. These difficulties were gradually surmounted by means of frictions and exercise, and at the present time, though he still experiences a little stiffness of the members, he can walk without assistance, and descends and ascends almost daily a long flight of stairs. The bowels have been, for some time past, in possession of their natural tone, and evacuate their contents without the aid of medicine. The general health of the patient is entirely restored.

John Hood, aged thirty years, was admitted into the Alms-house Hospital, on the 8th of June, 1830, affected with paralysis of the forearm and hand. The powers of motion were almost entirely destroyed, and sensation was so far extinct, that the part could be severely pinched without occasioning pain. He took the strychnine in the doses employed in the preceding cases, and by persisting in its use for about a fortnight, using at the same time frictions and pumping, he was relieved. The habits of this individual were intemperate, and it is highly probable that the affection of the arm proceeded from the influence of lead employed in adulterating the liquors which he drank.

Daniel Reardon, aged fifty-two years, came into the Hospital on the 20th of July, 1830, with the forearm and hand paralytic, and was put upon the same course of treatment which afforded speedy relief. At present he moves his arm and hand with great freedom, and only complains of a slight degree of weakness in the part. His habits are intemperate, and he states that he has been accustomed to drink very freely of common northern gin. We can assign no other cause for his disease than that which has been offered in the above cases.

In conclusion, we think it is undeniable, that in the above cases, the good effects of the strychnine were strikingly manifested, and that under similar circumstances, much may be expected from the judicious

employment of that article. We are at present using it in some other cases, but are unable, as yet, to determine what will be the result. We will take this opportunity of observing that we have employed strychnine with much apparent benefit in some forms of chronic irritation of the alimentary canal, especially in persons who were affected with habitual constipation of the bowels. It should, however, always be borne in mind, that its natural tendency is to irritate the mucous membrane.—*Amer. Jour. Med. Sciences. No. XIV.*

Abdominal pressure in Obstetrics.—The custom of applying a bandage or swathe to a female *after* accouchment, is general, if not universal. It is not, however, so commonly known as it ought to be, that the same application, vigorously made, is a great aid to the expulsive efforts of the uterus. Pressure by the hand of a friend is almost always inefficiently applied; and much time might be saved the practitioner, and much suffering the patient, if nurses were taught to press with sufficient force, and in the right direction, and at the proper times, on the uterus, during the pains of parturition.

The following statement from the London Review, contains a practical lesson on this mode of procedure.

Mr. M'Morris, a medical officer in the East India Company's service, writes to us as follows. "In one of the numbers of the Medico-Chirurgical Review for 1828, I find a bandage proposed by Mr. Gaitskell for obstetric purposes. I have no doubt it will facilitate the parturient powers of Nature by the support and pressure which it affords. It is a common auxiliary among the native women on this side of the Indian peninsula, when the progress of labor is slow. Some of the female attendants, in such cases, place their hands on the abdomen of the parturient patient, and press it pretty violently downwards towards the pubes, having first rubbed it with castor oil. This operation is continued till the child is expelled, which generally happens soon after the mechanical pressure is commenced. If the secundines are not expelled with the fœtus, the woman is left to rest, with a bandage applied pretty tightly round the abdomen. I have been astonished, in some cases that I have witnessed, to observe the increase of the expulsive pains soon after this process is put in force.—*Boston Med. & Surg. Jour. No. 4. Vol. 4.*

Stomach almost entirely converted into Cartilage.—The following

case, related in Rust's Magazine, is something of a poser to Mr. Halsted's old notion redivivus about kneading, shampooing, &c. &c.—A woman had had a tumor in the abdomen for twelve years; it was moveable, of a round shape, and had been unanimously declared, by several medical men who examined it, to be a scirrhous ovary. This patient, who had never had nausea, vomiting, nor any of the other symptoms by which scirrhus of the stomach is usually indicated, at length died; and on opening the body, it was found that the tumor was formed by the stomach converted into cartilage, which was an inch thick on the anterior surface, and it was only at the posterior part that a small membranous portion was discovered, less indurated and not so thick. The cartilaginous parietes of the viscus could not have performed any movement, and it would thus appear that motion on the part of the stomach is not essential to digestion. The preparation is deposited in the royal museum at Paris.—*Ibid.*

History of a case of Stammering, successfully treated by the long continued use of Cathartics. By John Bostock, M. D. F. R. S. [*Read March* 23, 1830.]—Impediments of speech are usually regarded as originating either in a physical defect of the organs which are exercised in the production of articulate sounds, or as proceeding from some cause more of a mental nature, as habit, imitation, or the like. The modes of treatment that have been proposed, as far as we are made acquainted with them, are accordingly adapted to one or other of these supposed causes, and consist either in certain methods of managing the muscles that are concerned in speech, or in counteracting those circumstances which may be supposed to induce the habit, or the tendency to imitation. A case has fallen under my observation, which has led me to take a different view of the subject; and as the treatment has been, upon the whole, successful, I presume that a short account of it will be acceptable to the society.

A boy of robust form and florid aspect, of a healthy constitution, and of more than ordinary activity both of mind and body, when between two and three years old, and after having acquired considerable readiness in speaking, was suddenly affected with so great a degree of stammering as to be almost incapable of uttering a single syllable. Two eminent physicians were consulted: they confessed their inability to propose any specific plan of treatment which might afford a prospect of success, but in consequence of a somewhat plethoric state of

the child, they advised that a strong purgative should be given. The effect of the medicine appeared so favourable, that it was repeated three or four times, and each time with such decided benefit, as to leave no doubt on this point in the minds either of the parents or the practitioners. The complaint, however, shortly recurred, was again attacked with the same remedy, and was again subdued. After this plan had been continued for some time, it was conceived that, in addition to the purgative system, the effect of which, although so salutary, was temporary, further advantage might be obtained by adopting a system of diet which should permanently reduce the plethoric habit, and obviate the necessity for the continual repetition of the purgatives. This was accordingly done, and was rigidly adhered to for several years. Animal food was totally abstained from, and even vegetables were taken in as sparing a quantity as was consistent with the support of the system. The effect of this regimen was sufficiently apparent in the altered aspect of the child, who became much less plump and florid, but still retained a due share of vigor and activity, and was fully adequate to enter into all the sports and exercises suited to his age. I had frequent opportunities of witnessing the result of this plan; and I may venture to assert, that it is impossible to have stronger evidence of the beneficial operation of any medical treatment, than is presented by the case in question.

By a steady adherence to this discipline for about eight years, the complaint was kept at bay; but whenever any relaxation in the diet took place, or when the purgatives were omitted or too long delayed, symptoms of the impediment immediately appeared. At length, when about twelve years of age, the tendency seemed so far subdued, that a relaxation of the restrictions was not followed by the usual unfavourable consequences, and the boy being then at a public school, it was not so easy to maintain the former discipline. For some time no bad effects ensued, but at length the complaint recurred, and was unusually obstinate, so as to require a long and severe course of purgatives, which, however, was finally successful.

During the last two years the tendency has occasionally manifested itself, but it has always been easily removed by a moderate use of purgatives, and by a temperate, although not a rigidly abstemious diet. The boy who is now in his fifteenth year, may be said to be free from the complaint. No one but those who are aware of the circumstances of the case, and were on the watch to detect even a slight defect,

would notice any thing peculiar in his mode of speaking. He even possesses a considerable rapidity and volubility of enunciation; and as a proof of this I may state, that I was lately present at a juvenile exhibition, when he bore a conspicuous part in a comic dialogue, in which he displayed a complete command over the organs of speech. In this respect, he may be favourably contrasted with many of those individuals who have been under the care of the masters who profess to remove these impediments. It would be unjust not to admit, that they occasionally produce very beneficial effects on those committed to their care, but I think it may be asserted, that in all these cases, there is a certain peculiarity in the mode of speaking, which although much preferable to decided stammering, indicates that the difficulty is rather evaded than obviated.

With respect to the purgatives employed in this case, it appeared to be of little importance which were used, provided the bowels were very completely evacuated. What was the most frequently employed was a full dose of calomel and jalap, succeeded by Epsom salts. Whenever the examination was made, it was found that the faeces were in a morbid state; and while the child was young, and the examination could be easily made, the necessity for continuing the medicine was judged of as much by the appearance of the faeces as by the state of the symptoms. It happened, on two or three occasions, that a degree of salivation was unintentionally excited, but it was not easy to determine whether this circumstance was productive of any advantage, as the relaxation of the bowels was contemporary and proportional.

How far we may be allowed to draw a general inference from a single case, I will not venture to decide; but I may be allowed to say, that a trial should be made of a plan of treatment which is productive of no inconvenience, which involves no expense, and does not interfere with education, or with the ordinary habits of life. It may be difficult to determine how far such a process should be recommended to adults. Much must depend upon age, constitution, temperament, &c., but I should suppose that few individuals would object to submit to a trial, although the hope of success may not be considerable.

As I propose this communication to be of a practical nature, I abstain from entering into any pathological observations on the nature and cause of the affection, further than to remark, that the complaint appears to consist essentially in a loss of power over certain voluntary muscles, and that as the muscles themselves do not seem to be affected,

it ought probably to be referred to the class of nervous diseases, and may be regarded as analogous to chorea, differing from it principally in its seat, and in its being confined to one set of muscles, while chorea affects a much greater number of parts, and produces a proportionally greater disturbance of the constitution and functions.—*Medico-Chirurgical Transactions*, 1830.

Fracture of the Cranium, accompanied with loss of Brain and Hernia Cerebri; successfully treated.—By Joseph K. Swift, M. D. of Easton, Penns.—"On the 16th December 1825, I was called to Jacob Shearrer, of this neighbourhood, who was thrown from his horse while in a state of partial intoxication, although his general habits were those of sobriety and industry. He had fallen against the sharp angle of a large stone, and received a fracture of the parietal bone, commencing about its middle, and extending downwards to the squamous suture, upwards to the sagittal suture, and backwards to the os occipitis, or very near to it. The wound of the scalp was between six and seven inches in length. About two square inches of the parietal bone were comminuted and driven through the dura mater into the substance of the brain: some of the fragments of the bone were visible; but most of them were hidden in the brain, being forced edgeways into it; one piece in particular, nearly an inch in diameter, was covered to half an inch in depth, and so completely concealed that I did not discover it until all the other fragments had been removed. I placed the pieces together, and finding that they would not fit, concluded that there must be a portion remaining, and found it with a probe. All the pieces so removed were entirely separated from the integuments.

"Three other fragments equal to two square inches, near the occipital bone, were turned at a right angle, directly into the substance of the brain, but these were not entirely separated from the scalp, although the dura mater and other membranes were lacerated. I elevated these portions of bone to their natural level.

"A surface of brain was thus exposed, from three to four inches square, in several places penetrated by fragments of bone to the depth of more than an inch, denuded completely, and lacerated by fragments of various sizes.

"After carefully removing these fragments, I separated nearly a table-spoonful of brain, which was lying loose upon the wounded sur

face; besides which, a small quantity of brain was observed upon the stone where the accident occurred, but how much I did not ascertain.

"A spicula of bone had penetrated the longitudinal sinus, from which a considerable quantity of blood had escaped previous to my arrival; it was then readily commanded by a dossil of lint. The left side was paralyzed, the injury being on the right.

"I drew the integuments together, and confined them by a couple of stitches, one at the upper and another at the lower angle of the wound, leaving a disunited space in the middle of about two inches. A light flaxseed poultice was directed to be placed over the wound; the patient's head to be kept greatly elevated, and cold applications to be applied, excepting to the part occupied by the poultice. I bled him three pints, gave a dose of cream of tartar and jalap. Diet: bread and water. Room to be kept dark, and absolute quiet enjoined.

"During the bleeding he vomited freely, and fainted twice. Notwithstanding the extent of injury which the brain had sustained, he retained almost entire possession of his senses; his feelings were excited and his memory somewhat affected, with an increased disposition to talk, but volition was not impaired. During the time I was operating on and dressing his head, he answered all questions in a rational manner; knew by the sensation when I removed a piece of bone, and conversed much with his wife and children.

"On the following morning the dressings were removed; numerous portions of brain adhered to them, perhaps a teaspoonful. A serous fluid was discharging. Bled him three pints.

"In the evening discharge of serum, mixed with blood. Bled him one quart.

"About the third day from the receipt of the injury, a portion of brain, equal in size to a common walnut, protruded from the integuments, by which it was considerably constricted at its base. Good pus was discharged in small quantity, and the tumour sloughed off in the course of three or four days, and was succeeded by two or three others of smaller size, which underwent the same process. For several subsequent weeks there was a copious secretion of healthy pus; and to be brief, the patient recovered without a single unpleasant symptom, to the utter astonishment of myself and all who saw him.

"The indications I pursued throughout this case were, the most rigid adherence to the antiphlogistic regimen; an upright position of the head; the constant use of cold applications; absolute rest and quiet,

and a careful preclusion of every exciting cause. Venesection was pushed to such an extent as to keep the pulse constantly below the fever point; whenever it evinced an increase of action I bled him, mostly *ad deliquium:* the bowels were slightly constipated, rendering purgatives occasionally necessary.

"The paralysis had much abated before the cicatrization of the wound and the slight disturbance of his mental faculties also subsided.

"Nine months have now elapsed since the occurrence of the accident, and the patient, a farmer, is able to attend to his business. His left side is still weaker than his right, although its use is completely restored. In August last, I called him into my office to show his head to my friend Mr. Franklin Peale, and in answer to the many inquiries we made him, he stated that he thought his faculties better than they were before his injury; that he could calculate with more facility, and that previously to the accident, he was subject to a very severe periodical headach, to which he is now a stranger.

"The motion of the brain is distinctly visible through the integuments, and the vacuity from loss of brain and bone would contain rather more than half a gill of fluid.

"May not the extent of laceration of the dura mater have been favourable to the fortunate issue of the case, by allowing a free discharge of pus and extravasated fluids, and consequently removing all pressure from the brain? At all events the case decidedly proves, that injuries of the brain, with loss of its substance, and consequent laceration of the membranes, are not necessarily mortal.

"March 21, 1826. The patient continues well, and experiences little or no difficulty in attending to his ordinary duties. The left arm is still not so strong as the right, and its motion not quite so free, but it is progressively recovering.

"My friend Dr. Cooper, of Easton, attended the patient with me after the second or third day of the accident."

Note. By Samuel George Morton, M. D., one of the physicians to the Philadelphia Almshouse Infirmary.

"Happening to be at Easton during the present month, (July 1830,) Dr. Swift politely took me to see the individual whose case is recorded in the preceding communication. Four years and a half have now elapsed since the accident occurred, and the subject of it remains precisely in the situation described by Dr. Swift: he assured me of his increased facility in making calculations, and of the entire absence of

headach, which had greatly afflicted him prior to the receipt of the injury. He is of a robust frame, and can at present endure as much hardship as at any former period of his life. No paralytic symptoms remain.

"The cavity in his head remains as great as ever, but the integuments have become much thickened, and the pulsation of the brain, though obscure, is still evident to the touch."—*North Amer. Med. and Surg. Jour. No. XXI.*

Abortion.—The following communication from a respected and intelligent medical friend, Dr. R. Lee Fearn, of Alabama, will be found interesting, and may prove of importance to the practitioner.

"I have a negro servant, who, in 1820 or 1821, was married, being then just thirteen years old. She soon became pregnant—and lost her child about the fifth or sixth month: she had lost five children at about the fifth month, at the time of her entering her sixteenth year, when she carried one to the seventh month or nearly. On consultation with several very respectable physicians, it was thought that a separation from her husband would be highly useful; they were accordingly separated at a considerable distance, for nearly two years, and we have reason to believe, with the utmost fidelity to each other. About five months after she came to her husband in this county, my brother and Dr. A. Erskine being in attendance, could not prevent an abortion; she continued to lose two a year, with no pain of any consequence, or other inconvenience, more than is usual for a few hours. At this juncture, I (being in the Philadelphia Alms-house) consulted Dr. Chapman, Dr. Dewees, Dr. Jackson, and others, and wrote to my brother, all to no avail. I found her, on my return home, again pregnant, with an extremely robust constitution, &c. and attended to her every day. She was delivered on the 25th of December last, at the full period of gestation, of a large boy, who is now alive, after having aborted ten or eleven times in nine years. My treatment in addition to what is usual and what had been used frequently with her, that is venesection, laxatives, absolute rest, &c. consisted in the internal use of the tincture of cantharides, in such quantities as to keep up continual strangury, and also in having the lumbar region over the spine rubbed with the terebinthinate decoction of cantharides, (an excellent article so frequently used in the Alms-house) so as to keep the part continually excoriated. When the strangury became excessive, I used the

usual means to mitigate it, and then continued the medicine."—*Ibid.*

Prolapsus Uteri—In the New York Medical Journal, Vol. I. No. I. Dr. Barker has made some judicious observations on the treatment of this troublesome complaint, and has proposed a modification of the gum elastic pessary, which, he says from experience, is very convenient and effectual. The indication is to keep the uterus in its proper position, without inconvenience to the patient. The more completely therefore the vagina can be filled without uneasiness, the more effectual will be the reduction; and experience would seem to prove that the globular or ovoid form is superior to any other. But practitioners are aware, that pessaries of this form are with difficulty introduced and removed, and hence one of sufficient size to pass the os externum will not always fulfil the requisite indications. Flat circular, and oval pessaries are hence substituted, and often effectually, but still in some instances completely fail. Mr. Barker therefore proposes a hollow gum elastic pessary of a pyriform shape, and as large as may be required, into the neck of which is to be inserted a small ivory tube containing a valve and spring, and surmounted by a silver cap with an orifice at its extremity. This is to be introduced in a flaccid condition into the vagina, which can be easily accomplished: then by means of brass tubes, straight and curved, furnished with stop-cocks, and of a common gum elastic injecting bag, atmospheric air can be forced into the pessary so as to distend it as much as may be necessary. The injecting bag and tubes being removed, the air will not escape, owing to the valve in the neck of the instrument. When desirous of removing the pessary, the valve is to be depressed so as to allow the air to escape, when the whole can be with facility extracted; should any difficulty occur, exhaustion of the pessary could be readily effected by means of the tubes and the elastic bag. The advantages of this pessary are, 1. Its ready introduction and removal. 2. Its capability of being distended to any required magnitude. 3. That the patient can herself introduce and withdraw it. And, 4. That its size can be very gradually diminished as the vagina contracts and gains tone, until it may altogether be omitted.

Dr. Barker says it might be removed at every menstrual period, and even oftener, say once a week, that astringent injections may be employed and the vagina allowed to contract.—*Ibid.*

On the Poisonous Effects of certain spoiled Articles of Food. [*Archives Generales de Medecine, Fervier*, 1830.]—It is well known that certain articles of food have been frequently observed on the Continent to acquire poisonous qualities of a peculiar kind, and in a way which chemists and physicians have not hitherto been able to explain very satisfactorily. Among these articles, the most frequent are a peculiar variety of sausage, and a particular kind of cheese, used in Germany; but both in France and Germany, bacon and ham have been also several times found to acquire poisonous qualities analogous to those which characterize the sausage poison and cheese poison. A very elaborate inquiry into an accident supposed to have arisen from spoiled ham, has just been published by M. Olivier in the Archives Generales de Medecine. His investigations set completely at rest the common notion that such accidents arise from the accidental impregnation of the meat with metallic poisons; but he has not succeeded in discovering the real cause.

In the instance which gave rise to his investigation, the master of a family purchased a ham pie at a pastry-cook's in Paris, and the whole family ate thé meat of the pie the same day, and the crust on the following day. Three hours after dinner, the master of the house was seized with general uneasiness, followed by cold sweats, shivering, violent pain in the stomach, and frequent vomiting; then with burning thirst, extreme tenderness of the belly, so that the weight of the bed-clothes could scarcely be borne, profuse purging and colic of extreme violence. His daughter, twenty-seven years of age, and a child nine years old, were similarly attacked. A physician who was called to their assistance soon after they were taken ill, drew up a minute report of the symptoms in each of his patients, and declared that they had a violent inflammation of the stomach, which he was inclined to ascribe to natural verdigris, or the carbonate of copper, having been communicated by the pastry-cook's moulds. In a few days all the three individuals recovered, under an antiphlogistic treatment. About the same period, several accidents of the like nature occurred among the customers of this pastry-cook; and in consequence a judicial investigation was ordered. The shop being properly inspected, it was found that every operation was conducted with a due attention to cleanliness. MM. Ollivier and Barruel were appointed to analyze the remains of the meat which produced the cases first mentioned, as well as the alvine discharges of the child.

Their experiments furnish ample proof that the pie did not contain a trace of arsenic, copper, antimony or lead. The only conclusion, therefore, which M. Ollivier conceived it possible to draw, was, that the ham had in some way or other acquired the poisonous properties sometimes remarked in German sausages, cheese, and ham.—*Edin. Med. and Surg. Jour.*

Remarks in relation to the use of the Trocar in paracentesis abdominis. By Jabez W. Heustis, M. D., *of Alabama.*—Whoever has performed the operation of paracentesis of the abdomen with the trocar in ordinary use, must be aware of the degree of difficulty attending it. The operation is simple and unimportant in itself, but when the trocar is pushed against the parietes of the abdomen, considerable force is required to make it enter. This comes partly from the abrupt bluntness of the trocar, but principally from the resistance exerted by the canula; the surrounding parts closing upon and embracing the perforator, and thus refusing admission to the instrument without the use of considerable force. Having uniformly experienced these difficulties in the use of the trocar, of which I have used those of the best construction, I have for a considerable time laid this instrument entirely aside in all cases of ascites requiring puncturing. As a substitute, I make use of the common thumb lancet; in the distended state of the abdomen this is introduced without the least difficulty, and contrary to what happens with the trocar, causes little or no pain. For a canula, to conduct the water, I make use of a quill, cut off at both ends. A silver canula, with one end closed, and a small opening on each side, would be more elegant, but not having an instrument of this description, I have found the quill answer every purpose. The superior advantage and facility of this mode of operating need only to be tried to be fully appreciated. I claim but little merit in the plan here described; presuming that others may have previously adopted the same; being a practice that would obviously suggest itself to every one who has had any share of experience in paracentesis abdominis.—*New York Med. and Surg. Jour. Nov.* 1830.

Case of Monstrosity, by Mark Stevenson, M. D., *of New York.*—Mrs. E., aged 40, of a delicate constitution and scrofulous habit, was married at an early age and had several healthy children.

After this her husband died, and she remained a widow six or seven years.

In the month of September, 1827, she had enlargements of the submaxillary glands, which suppurated, and discharged much matter; under appropriate remedies they were entirely healed, and have never made their appearance since.

On the 24th of August, 1828, she was again married, soon after which she had all the symptoms of pregnancy. The uterus continued to enlarge much more rapidly the first four or five months, than it did the succeeding period of gestation.

Her symptoms during the latter period of pregnancy were of a singular nature: viz. frequent rigors, producing an agitation of the whole system; these were succeeded by flashes of heat, quick pulse, &c. She complained of a cold, heavy and dead weight about the uterine region; and whenever attempting to change her position in bed, the uterus with its contents, would seem to roll from one side of the abdomen to the other, as if in a detached state. Prior to her delivery, she had several attacks of violent flooding from the uterus. She frequently remarked to her friends, that she never had experienced such a train of distressing sensations during her previous pregnancies.

On the 27th of June, 1829, she supposed herself in labor, it being about the close of utero-gestation. At this time I was summoned to see her, and on my arriving at the house the nurse stated to me that something had come from the patient; but that it was not a child. I immediately repaired to the bedside; and upon examination found that a substance had been expelled, of the size and appearance of a large bullock's bladder. From a more accurate examination of the substance, it proved to be a regularly formed sack, gradually decreasing in size from its fundus, and terminating in a well formed cervix, having an orifice, at which part its coats were much thickened, affording a surface for attachment to the uterus.

The sack had three distinct coats, a membranous, muscular, and villous coat. At the orifice of the sack there was a fatty substance projecting, which I drew out. It was of an adipose structure, partaking somewhat of the appearance of gland. It also possessed some uniformity of shape, having a body and extremities, but in no respect organized. In addition to this, the sack contained about two quarts of very fetid bloody matter.

Doctor Belden, of this city, was called in to witness this curious

phenomenon; and to his care the patient was intrusted, as I then left the city for a few days.

After delivery there was considerable fever for a few days; but by the use of judicious and appropriate remedies, it was soon subdued; and in the course of one week she was perfectly restored.

The substances expelled are now in my possession, preserved in nearly as perfect a state as when first obtained.—*Ibid.*

TO CORRESPONDENTS.

In a recent number of the American Journal of the Medical Sciences, for February, 1831, the Transylvania Journal of Medicine is graced with a lengthy and detailed notice of two very short cases, published in the *May No. for* 1830: and "for the gratification of our contributors," Drs. Cloud and Fletcher, we refer them to the aforesaid No. of the American Journal for the reperusal of their cases, together with the sage remarks thereon;—and for the information of our readers generally, who may not have noticed the particular cases alluded to, or may not be so fortunate as to possess the 'American,' we have to state that one or more of its *editors*, *collaborators* or *contributors*, (W. P. D. &c.) have very earnestly cautioned the public against the fatal effects likely to result from the novel practices pursued in the aforesaid cases; which practices they have very graciously been pleased to intimate have been "presented, if not under the avowed sanction, at least under the countenance" of the editors of this Journal! Verily, the aforesaid W. P. D. &c. have been in a great haste to discharge their *duty*, by entering this protest against such mal-practices, seeing that nearly a whole year has elapsed since the publication of these cases, before they caution the world against their fatal tendencies; or they have taken full time for due deliberation, before they have ventured upon their veto. If, however, our sapient advisers are saddled with the onerous burden of *countenancing* or *sanctioning* all the principles or practices promulgated in our "respectable cotemporary" of the East, we should, in the charity of our nature, find ample excuse for such leisure and delay; for we must confess they have a hard *duty* to perform. For ourselves, we now, once for all, assure them and their readers, as well as our own, that we assume no such responsibility

upon ourselves; nor do we, as editors, pretend to sit in judgement upon the theories or practice of our contributors, however discordant they may be with our own; but freely and fearlessly give publication, through our pages, to whatever may be offered us in a respectable shape, and under a responsible name, trusting to the good sense and discernment of the profession for their due appreciation.—*Edrs.*

OBITUARY NOTICES.

Died, in this town on the 19th of the present month, March, Mr. JAMES M. PARRISH, student of medicine, formerly of Lexington, but latterly of Elkton in this State. His illness was of long continuance, about three months, and he bore it with exemplary patience. His conduct throughout was such as to secure, in an extraordinary degree, the affectionate and sedulous attention of his room-mates and other occasional attendants. And his friends and relations at a distance will no doubt be gratified with the information, that by the influence of one of his class-mates, his attention was so effectually turned, before the commencement of his illness, to the subject of religion, that his chief pleasure during his confinement, was reading the Testament or hearing it read, and religious conversation; and that he died resting his hopes of future happiness on the merits of the Saviour alone. C.

—— In this place, at the residence of his father, Dr C. W. Cloud, on the 1st March, inst., Mr Thiophelus W. Cloud, aged 19, late a member of the Medical Class. We had not the pleasure of a particular acquaintance with Mr C., but from the testimony of his friends we believe him to have been a young man of amiable manners and of studious habits. He profest to have peace with his God, and died like a christian.

UNIVERSITY INTELLIGENCE.

MEDICAL GRADUATES, 1831.

At a public commencement held in the Medical Hall, of Transylvania University, on Thursday the 10th day of March, 1831, the degree of *Doctor of Medicine* was conferred on the following gentlemen, who submitted Dissertations on the subjects annexed to their names, viz.

1. Charles W. H. Alexander, *of North Carolina*. Thesis De Ingenita tractatione antecedente et subsequente Chirurgiæ; et ejus momento.
2. Shadrach Bell—*Tennessee*. On Curved Spine.
3. Barry W. Benson—*Mississippi*. On the Diseases of Dentition.
4. Anderson M. Berry—*Kentucky*. On Typhus Fever.
5. Thomas C. Black—*Tennessee*. On Rheumatism.
6. Boling A. Blakey—*Alabama*. On Mercury; and the Treatment of Mercurial Salivation.
7. Burwell R. Bobo—*South Carolina*. On Phrenology as applied to Pathology and Surgery.
8. Hugh S. Bodley—*Kentucky*. On Paronychia.
9. James A. Bullock—*South Carolina*. On Dyspepsia.
10. Swan P. Burnett—*North Carolina*. On Typhus Fever.
11. Newton L. Cain—*Georgia*. On Fractures of the cervix of the Os femoris.
12. William M. Carper—*Virginia*. On Dysentery.
13. Robert F. Cooke—*Tennessee*. On the Sanative influence of Biliary evacuations elicited daily in the Treatment of Chronic Diseases.
14. Richard C. Crump—*Virginia*. On Digestion.

15. Robert M. Daniel—*South Carolina*. On Digestion.
16. James W. Dunklin—*Alabama*. On Puerperal Fever.
17. Samuel T. Feamster—*Virginia*. On Emetics.
18. Washington W. Geiger—*South Carolina*. On Congestive Fever.
19. Richard C. Griffin—*South Carolina*. On the Bilious Intermittent Fever of the South.
20. Thomas B. Grigg—*Virginia*. On the Non-contagious nature of Rubeola.
21. William B. Hamilton—*Kentucky*. On Caries of the Teeth.
22. John W. Hanna—*Alabama*. On Epidemic Fever.
23. Charles R. Irvine—*Virginia*. De Diagnose Erysipelatis ab Phlegmone et Erythemate.
24. Robert E. Jones—*Virginia*. On Puerperal Fever.
25. Frederick Jones—*Alabama*. On the Endemic Fever of Madison County, Alabama.
26. Thomas Littlejohn—*South Carolina*. On Digestion.
27. Carey L. Lesueur—*Georgia*. On the Autumnal Bilious Fever of Southern Climates, with its Pathology and Therapeutics.
28. Robert C. Malone—*Tennessee*. On Inguinal Hernia.
29. Nathaniel E. McClelland—*North Carolina*. On Cholera Infantum.
30. Preston B. McGoodwin—*Kentucky*. On the use of Emetics in Indigestion.
31. John R. MacLeod—*Alabama*. On Bronchocele.
32. Samuel G. Menzies—*Kentucky*. On Dysentery.
33. William B. Moores—*Tennessee*. On a Remittent Fever which prevailed epidemically in Smith County, Tennessee, in the autumn of 1830.
34. Madison Montgomery—*Georgia*. On Syphilis.
35. John T. Nolan—*Alabama*. On Opacity of the Cornea.
36. James O'Brien—*Ireland*. On Hydrophobia.
37. Joseph J. A. Pugh—*Tennessee*. On the Physiology of the large Intestines.

38. Edwin T. Rochester—*Kentucky*. On Acute Dysentery.
39. Alfred E. Saunders—*Virginia*. On Hydrocephalus.
40. Mark H. Scales—*Tennessee*. On Amenorrhœa.
41. James E. Scott—*Alabama*. On Dysentery.
42. Thomas Smith—*Kentucky*. On the Application of the Bandage in the Treatment of Fractures.
43. William S. Smith.—*Mississippi*. On the Physiology of the Brain.
44. Samuel Snoddy—*South Carolina*. On Digestion.
45. Buford A. Spurr—*Kentucky*. On the Non-contagion of Yellow Fever.
46. Hiram J. Thomas—*Tennessee*. On Morbus Coxarius.
47. Rufus A. Wallace—*North Carolina*. On Congestive Bilious Fever.
48. Henry Y. Webb—*Tennessee*. On Diabetes.
49. Charles Williams—*South Carolina*. On the Sympathies of the Stomach.
50. John P. Williams—*Tennessee*. Erysipelas.
51. Joseph C. Woodson—*Kentucky*. On Cachexia Africana.
52. Elijah J. Young—*Kentucky*. On Prolapsus Uteri.

The *Honorary degree* of Doctor of Medicine was conferred upon James King, of Knoxville, Tennessee; and Pinckney C. Caldwell, of Charlotte, North Carolina.

C. W. SHORT, M. D. Dean.

APPOINTMENT TO THE CHEMICAL CHAIR.

The Professorship of Chemistry in the Medical Department of this Institution being vacated by the resignation of Dr. Blythe, on the 16th of March 1831, the Trustees promptly proceeded to supply the vacancy; and, at a meeting held on the 26th, elected Lunsford P. Yandell, M. D., of Nashville, Tennessee, Professor of Chemistry, and H. Hulbert Eaton, A. M. (R. S.) late Junior Professor in the Rensselaer School of Natural Sciences of New York, Assistant Professor of Chemistry.

ADDITIONAL SUPPLY OF BOOKS, APPARATUS, &c.

Professor Dudley, who is now in Europe, where he will spend the present recess, has been amply supplied with funds for the purchase of books, apparatus, &c. So that on his return in the fall the Medical Library and Chemical Laboratory, will be enriched with many valuable additions: and Students attending the succeeding course of lectures will, moreover, have the advantage of receiving much of the recent medical and surgical intelligence of the European Capitals.

SUMMER COURSE OF LECTURES.

The Summer course of Medical Lectures in Transylvania University will commence about the middle of May and terminate towards the end of August.—The course will embrace a general view of the principles and practice of Medicine.

THE

TRANSYLVANIA

JOURNAL OF MEDICINE

AND THE ASSOCIATE SCIENCES.

VOL. IV. NO. II.——WHOLE NO. XIV.

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NOTICES.

Since the publication of our last number, we have received,

1. The American Journal of the Medical Sciences, No. XV, May, 1831. (In Exchange.)

2. The North American Medical and Surgical Journal, No. XXII, April, 1831. (In Exchange.)

3. The American Journal of Science and Arts, conducted by Benjamin Silliman, M. D. L. L. D. &c. No. I, Vol. XX, January, February and March, 1831. (In Exchange.)

4. The Journal of Health, conducted by an Association of Physicians in Philadelphia, twice a month; Nos. 14 to 20, Vol. II, inclusive. (In Exchange.)

5. The Journal of Law, conducted by an Association of Members of the Bar in Philadelphia, twice a month; Nos. 18 to 24, inclusive. (In Exchange.)

6. The Illinois Monthly Magazine, conducted by James Hall; Nos. 6, 7 & 8, March, April and May, 1831, Vol. I. (In Exchange.)

7. The Boston Medical and Surgical Journal, weekly; Nos. 6 to 19, inclusive, Vol. IV. (In Exchange.)

8. The New-York Medical Journal, conducted by Daniel L. M. Peixotto, M. D. John R. Rhinelander, M. D. John J. Graves, M. D. Nos. II and III, February and May, 1831. (In Exchange.)

9. American Annals of Education and Instruction, and Journal of Literary Institutions; conducted by William C. Woodbridge, Boston; Nos. 4, 5 and 6, Vol. I, monthly. (In Exchange.)

10. The Southern Review, Charleston, S. C. No. XIII, May, 1831. (In Exchange.)

11. The Maryland Medical Recorder, conducted by Horatio G. Jameson, M. D. &c. No. I, Vol. II, April, 1831. (In Exchange.)

12. An Essay on the History, Preparation and Therapeutic uses of Iodine. By Samuel G. Hopson, M. D. (From the Author.)

It will be remembered by the readers of this Journal, that in June, 1830, a Prize was offered by the Medical and Surgical Faculty of Maryland, for the best Dissertation on the nature production, effects, and modes of preventing and destroying the Malaria which gives rise to bilious fever. The Prize has been lately awarded to an Essay on the subject by Professor Caldwell.

About the same time a Prize was offered, by the Medical Society of N. York, for the best Dissertation on *Delirium tremens*. That Prize has been recently adjudged to an Essay by James C. Cross, M. D.

THE

TRANSYLVANIA JOURNAL

OF

MEDICINE

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For April, May and June, 1831.

ORIGINAL COMMUNICATIONS.

ARTICLE I.—*Remarks on Hydrocephalus.* By JOHN P. HARRISON, M. D. of Louisville, Kentucky.

THE disease which I am about to remark upon, is one of deep interest to every practical physician. It has awakened a keen and persevering investigation wherever practical medicine is cultivated with any ardor of inquiry. The light of disciplined observation, and of well conducted pathological research has been shed with increasing lustre upon the nature and treatment of hydrocephalus since the days of Whytt, who entertained some very erroneous views in reference to the pathology and treatment of this disease. The usual division of hydrocephalus, is, into external and internal: but my object is to confine my reflections to the latter exclusively, as hydrocephalus externus is a very rare form of the affection, and admits of little or no interference on the part of the medical

practitioner. Cases, I am aware, are on record, of a cure having been accomplished, after immense accumulations of water had taken place between the membranes of the brain, by tapping, and the subsequent application of a well adjusted bandage to the head. Such novelties stand like oases in the midst of a desert, They are objects of pleasing admiration, but admit of very little application to any useful practical design.

It is not to be expected in an essay of this kind that any pretension is put forth after any novelty of pathological views on a disease so well understood by the profession as hydrocephalus. A careful analysis of the many valuable facts to be found in the works of Cheyne, Rush, Cooke, &c., is all that is contemplated. A concentration of the scattered rays of light which beam from the numerous publications on this subject, will essentially conduce to the best interests of the profession in directing the mind of the reader to the most accurate deductions, and in conducting his decisions of judgment to the true nature and treatment of the disease.

Hydrocephalus, or hydrencephalus, for it is known by both appellations, though the first is the generally received one, is divisible into idiopathic and symptomatic.

By idiopathic hydrocephalus is meant, a primary affection of the arachnoid and pia-matral tissues, or of the substance of the brain itself, which may or may not terminate in effusion of water. It partakes too much of the spirit of cavilling and verbal disputation to be forever indulging in a querulous strain against medical nomenclature. The inaccuracy of human language, its want of definiteness, its ambiguity, its misapplication, and other abuses are fruitful topics of complaint on the part of those who, perhaps, are inclined too much to verbal criticism. It is conceded that nomenclatural distinctions are absolutely essential to the explication of all scientific views. But a hypercritical attention to the accuracies of language fritters the attention of the mind, and sets it in search of objects of an import not commensurate to the demands of an enlightened regard for practical truths. I, therefore, prefer the

term hydrocephalus, however contradictory the derivative signification of the word may seem, to a veritable exposition of the pathology of the disease. Words are not to be understood in their original, but generally received meaning. Hydrocephalus, as it presents itself in its idiopathic form, is of less frequent occurrence than as it supervenes upon the progress of some other affection, or results from the transfer of irritation from some other organ. It has been denied by high authorities that it ever occurs as an idiopathic malady—as a disease whose primary seizure is on the brain. This is surely carrying the doctrine of sympathy to an ultra degree, far beyond the sober region of just observation. For why cannot the brain, or its meninges be the points of a primary inflammation as well as other organs, or tissues? The "one eyed monarchs" of modern medicine seem disposed to see nothing but the stomach, or liver, in all their researches of pathological anatomy. But like all exclusives in science they move in a circle of restricted observation, instead of advancing on the high-way of progressive attainment. The passion of medical philosophers for some system by which every phenomenon in disease can be explained is a striking proof of the imperfection of their reasoning. In an apparent simplification and generalization of doctrine, there is, it must be acknowledged, something, at first sight altogether alluring. It saves the mind from the painful process of reflection, and amuses the judgment with analogy, when it looked only for the severity of logical discussion. It is an evidence that medicine is still in its noviciate in philosophy, whilst such universal, all explaining, and at the same time, narrow systems are so confidently promulgated by their originators, and take such a quick lodgment in the minds of medical men. Idiopathic hydrocephalus generally attacks children under ten years of age, though there are instances on record where it invaded the brain of adults. It may be regarded as acute and chronic, according to its duration.

The disease seems hereditary in some families, viz. there seems to exist a predisposition, or peculiar tendency in the brain and its membranes to take on the inflammation which termi-

nates in effusion, upon the application of some irritating cause, either directly to the head, or through the functional disturbances of more distant organs, especially the chylopoietic. Dr. Cheyne relates instances of this kind, and states that he has heard of an unfortunate father who had lost ten children by this malady. Children from the fifth or sixth month to the ninth or tenth year, are the most common victims of the disease. Its subjects, more particularly when it is symptomatic, are generally of the strumous diathesis. In children of this temperament the capillary system of all the tissues is more excitable, and more liable to take on a species of inflammation which partakes much less of the character of a restorative process, than those who are exempt from its peculiarities. The skin, as an organ of sensation and secretion, holds a powerful control over the healthful performances of every tissue of the economy. In the scrophulous diathesis it is stamped with obvious indications of sensibility. It is not very tolerant of cold, and is imbued with a great liability to inflammation, in the subjects of this temperament. The cellular membrane, which is diffused throughout the entire organization of the frame, evidently participates in the peculiar tendencies of the skin, as evinced in subjects of this diathesis. Perhaps the whole organization of the strumous diathesis is modified by the developement and activity of the cellular tissue. The lungs in persons of this diathesis are liable to that fatal form of pulmonary disease, called phthisis pulmonalis, which most commonly attacks adults—though many young persons are destroyed by it. In strumous children the lungs are very liable to attacks of inflammation, which carry off many every year. The mesenteric glands are often incurably diseased in persons of this peculiarity of organism. There seems no tissue, no organ in the body which escapes the ravages of scrophula. The brain and its investments are often the seat of various attacks of acute and chronic disease in persons of the strumous diathesis. There is a great susceptibility to irritation in all the tissues in the infantile period of life. This susceptibility is heightened by the enhancement of organic sensibility arising from the scrophu-

lous temperament. Hence, from the conjoint agency of these two causes, children of this diathesis are exceedingly liable to attacks of the brain, both idiopathic and symptomatic.

The subject of dropsy presents a wide field for curious and interesting research. That field has been ably trod by Parry and Ayre. The trite common-place reflections about debility, as being the cause of dropsical effusion are melancholy proof of the slowness with which pathological truth penetrates prejudiced views of diseased action. But I must dismiss this consideration, and continue the thesis more immediately under discussion. That hydrocephalus depends on an inflammation of the arachnoid and pia-matral membranes, or of the cerebral substance, is now so universally conceded as to need no elaborate argument to prove. But it is not so generally conceded, that irritation, previous to the developement of those organic signs in the membranes or brain which characterize inflammation, may give rise to all the symptoms of the disease, and may even cut off the patient, and yet upon dissection no vestige of structural lesion be apparent. That the vital properties of an organ may be destroyed by local irritation, or that the entire compages of the organism may be suddenly rendered unfit for the purposes of life by constitutional irritation, no sober observer of facts can doubt. Thus the sudden introduction of cold water into the stomach of a man exhausted by labour may arrest its functions, and induce death. A blow ab extra inflicted on the stomach will do the same. Powerful emotions of mind will deprive the brain of its functional properties, and thus produce death. The reader is referred to the work of Travers for some very striking facts of the serious results flowing from constitutional irritation. The irritation of the meninges or of the substance of the brain, which precedes all cases of symptomatic hydrocephalus derives its origin from the primary affection of some distant organ. Thus hepatic derangement, or gastric disturbance will produce that irritation, which unless arrested by medical treatment or by death, runs on to inflammation, which often, though not invariably terminates in effusion.

Hydrocephalus, when idiopathic, is ordinarily ushered in by the following group of symptoms. The child becomes listless and dull—the eyes are watery, or injected with blood; if it attempts to enter upon the common sports of childhood, it does so with seeming reluctance, and suddenly abandons them; it is fretful and wants to be nursed or to lie on the bed; sudden pains dart through the head, which make it scream out with a piercing cry; the eyebrows become habitually contracted and knitted together, the pulse is full, the skin hot, the bowels constipated, or in a state of diarrhœic irritation, the discharges indicating depraved secretion of the liver, or an entire destitution of bile—when the head is raised, if the child be in bed, it manifests a wish to lie down and not to have the head disturbed—the tongue is generally furred, as should be expected in such an attack. The disease is sometimes suddenly ushered in by convulsive action of the muscles of the body, and death speedily occurs. I have known children die within a few hours from the seizure, when hydrocephalus thus made its onset. No effusion takes place in such instances—the powerful irritation extinguishes life before even inflammation is developed.

The division of the disease into two stages; 1st, the stage of excitement, and 2nd, that of effusion; will avail but little in the correct treatment. For the symptoms of effusion are often induced by the turgescent condition of the capillaries of the tunica arachnoides, pia mater, and brain, independently of the least effusion. The researches of Serres demonstrate this beyond all contradiction, although I by no means accede to all his deductions. Cases simulative of effusion arise frequently from some distant irritation, such as worms or indigestible ingesta in the alimentary canal, and general exhaustion of the system brought on by hypercatharsis. Dr. Marshall Hall has denominated the affection arising from exhaustion, which is characterized by coma and other symptoms of oppressed brain in young subjects, hydrencephaloid. We often witness this simulation of hydrocephalus in the last stages of the cholera infantum. In a paper published by me in the 1st No. of this journal on cholera infantum, a doubt was started whether the

hydrocephalic symptoms present in the last stage of that disease depended on either inflammation or effusion in the brain. It affords me great satisfaction to have my doubts converted into reality by the opinion of a man so gifted with medical perspicacity as Marshall Hall. Dissections reveal the existence of injected capillary vessels in the arachnoid and pia-matral tissues, and the brain, in those cases where the patient was cut off previous to effusion of water. In a few instances no traces of organic alterations were ostensible. In more chronic and subdued forms of the disease the substance of the brain has been found soft and blanched, and of a fimbriated appearance towards its base—the ventricles being filled and distended with water, and effusions of water under the tunica arachnoides. In very protracted cases the mass of the brain has become quite soft and pulpy. Dr. Delafield has given a case in one of the early numbers of the Philadelphia Medical Recorder in which no part of the brain was visible, but a small mass at the basis of the skull, which appeared to be the pons varolii. The extremities of the nerves were seen floating on a pulpy semi-fluid matter which covered the basis cranii. Notwithstanding, says the Doctor, the greater part of the brain was completely destroyed, and its place supplied only by a volume of fluid, this child not only lived, but appeared to have possessed during life the full enjoyment of all its senses. Dr. Horner, in his Pathological Anatomy, has given a very interesting analogous case, and others may be found in Good and other authors. Such cases are irremediable, and therefore reflect no light on the treatment.

Idiopathic hydrocephalus may arise from concussion, or direct injuries inflicted by blows, or falls, upon the head. Rush has given several apposite instances illustrative of the fact that the disease may originate from such causes. Cheyne seems to undervalue this view of the etiology of the disease. But he evidently was controlled too much by his abstract pathological principles. We every day witness in our science the misleading influence exerted by exclusive views as regards the pathology of disease. Morbid anatomy is excellent beyond all

estimate, in its place, but it should never usurp the entire dominion of our practical researches into the nature, seat, and cure of diseases. The history of the complaint under the consideration of the practitioner, with the premonitory and present symptoms, and the effects of the remedies administered must all be weighed in the scales of a sober judgment. Post mortem appearances taken in connexion with the symptoms of the affection during life, and the mode of treatment, are valuable in casting a guiding light on the path of practical medicine; but taken alone, without such reference to the previous history and treatment, they only create misconception on the true character of the diseases.

In the affection under discussion, the pathology cannot be satisfactorily made out from a restricted contemplation of the organic lesions seen either in the brain, or in the viscera of the abdomen, or in both of these divisions of the body. The turgescent condition of the blood vessels of the brain and its investments, may be but secondary upon the previous morbid actions, originating in the chylopoietic organs; or the structural alteration visible in the latter may be only consecutive and consequential upon the anterior cerebral affection. Keeping this important consideration in view, we shall be constantly guarded from rash inferences respecting the real pathology of hydrocephalus. The idiopathic form of this disease when suffered to progress in its downward career will implicate the viscera of the abdomen in a manner that may mislead the unwary practitioner, and turn off his eye from a careful inspection of the substantive fons et origo of the complaint. The symptomatic derangement often becomes a more prominent object of regard than the primary. When the brain is deeply disturbed in its functional agency, as the fountain head of nervous influence, it quickly imparts a corresponding embarrassment to the functions of every organ of the body. The liver and intestinal canal being thus acted upon by a withdrawment of nervous influence from the morbid condition of the brain, soon become foci of irritation. Thus it is that in idiopathic hydrocephalus the intestines are with much difficulty roused into a

due performance of their peristaltic action, and in the alvine excretions procured by purgative medicines there is great vitiation of hepatic secretion evinced, or, what is more to be deprecated, an entire absence of bile witnessed.

As hydrocephalus advances other symptoms reveal themselves. Paralysis of an arm, or an arm and leg, complete coma, strabismus of one or both eyes, from the preponderance of action in the internal oblique muscles, or instead of coma there may be delirium and piercing cries suddenly intermitted, a sawing motion of an arm or leg, or of both sometimes, comes on, or the patient may become cataleptic, as I recently had an opportunity of witnessing. There are several affections which so closely simulate idiopathic hydrocephalus as not to be easily distinguishable. Verminose irritation, especially from ascarides, produces the most prominent phenomena of a serious derangement of the encephalon. On several occasions I have been mistaken, as regards the cerebral affection, by the irritation of worms. The strabismus, coma, and full pulse were all present in one case. In another, violent convulsions, unabated by a copious detraction of blood, was the alarming form in which this intestinal irritation simulated hydrocephalus. In both cases a full dose of calomel, which induced an evacuation of the worms, removed the alarming appearances. Dentition sometimes gives rise to convulsions. Of course the practitioner must advert to the condition of the gums, and freely scarify them in such a case. Ingesta of an unwholesome kind, or an excess in the quantity of food, however congenial to the infantile powers of digestion, will often bring on the most terrific phenomena of diseased encephalon. Articles of an acescent kind, which are most likely to be taken in excess, are most apt to create this irritation; apples, plumbs, cherries, peas, beans, currants, &c. are at times the cause of death from the great irritation created by their presence in the stomach. Violent febrile excitement will sometimes produce convulsions and other symptoms of the hydrocephalic affection. The orgasm of fever in such a case will, by acting directly on the brain endanger the integrity of that organ, unless prompt mea-

sures are adopted for the reduction of the cerebral determination. Even where no convulsive symptoms are present, the febrile orgasm often in children induces coma, &c. that often excite great alarm in the minds of the parents. In such instances the coma and other phenomena simulative of hydrocephalus go off with the fever, without producing any serious effects.

In attacks of intermittent fever occurring in children, this is often witnessed. The treatment is to be varied as the stages of the intermittent alternately present themselves. Wherever, in the hot stage of ague and fever, symptoms of hydrocephalus are seen, blood may safely be drawn, and cold applications made to the head. Symptomatic hydrocephalus is often simulated in the last stage of several protracted infantile complaints. So frequently have I been foiled in the treatment of the apparent cerebral affection which closes so often the sufferings of a patient with cholera infantum, that I am determined to pursue an opposite course of treatment hereafter. Marshall Hall in his valuable works "On the Morbid and Curative Effects of Loss of Blood," has given several highly instructive cases in point. Dr. Abercrombie in his "Researches on Diseases of the Brain and Spinal Cord," and Mr. Travers in his work "On Constitutional Irritation," have made some very judicious reflections on the symptoms imitative of serious cerebral disease which arise from exhaustion in children, whether that exhaustion proceed from excessive catharsis, or from a surgical operation. My design in this paper is foreign from quotation: this reference must, therefore, satisfy the reader, who has much valuable information to acquire on this subject if he has not already consulted those eminent authorities.

When symptomatic hydrocephalus arises from hepatic derangement, to which source Cheyne almost exclusively refers it, the symptoms are those indicative of a depraved secretion of the liver. Costiveness, alternated with looseness, the excretions being sometimes clayey in their aspect, or tar-colored, with tumid abdomen, defective appetite, foul breath, furred tongue, and a dull watery eye are certain indications of great functional disturbance of the chylopoietic viscera.

Recently I saw a case in which the cerebral malady supervened on slight bronchitis. Repelled eruptions are sometimes the cause of the affection. Abernethy says he has seen cases in which every phenomenon of hydrocephalus was present during life, and yet on dissection the brain manifested no diseased appearance, but in which the intestines were greatly inflamed. Cheyne remarks that this phlogosed state of the mucous coat of the bowels is the effect of the state of the liver. But, perhaps, the brain in such cases may have been primarily diseased, which, by the impairment of the nervous functions of that organ, transmitted an irritation to the liver and intestines. The sympathy or consensus partium between the brain and chylopoietic organs must be reciprocal; therefore, whether the disease commence in the brain or in the abdominal viscera, irritation will be experienced in the consenting or sympathizing organ. Marasmus, Hamilton observes, frequently terminates by the superinduction of dropsical effusion in the ventricles of the brain. Parry would explain such a mode of fatal termination, not by the introduction of the talismanic word debility, which a distinguished author terms *asylum ignorantiæ*, but by referring it to the increased momentum of the capillaries of the brain, induced by the irritation imparted by the diseased state of the abdominal viscera. Dropsy of the brain, like any other hydropic affection, whether of the thorax or abdomen, or cellular tissue of the extremities, may arise from the inflammatory action of the capillary exhalents of the part whence the effusion comes, without any very manifest indications of an increased action in the heart and arteries, or this local exalted power of the vessels may be connected with general excitement.

In idiopathic hydrocephalus we have the local diseases connected with obvious manifestations of an excited state of the heart. In the symptomatic form of the affection this general excitement may, or may not be present. It is not to be understood that I consider the local affection always the effect of the general increased action of the heart: in idiopathic hydrocephalus the local affection in a majority of cases pre-

cedes the excitement of the heart; but in the symptomatic form the irritation or inflammation of the membranes and brain succeed the deranged condition of the diseased organ with which they are brought into consent of morbid movement. The disease, as I have already stated, may prove fatal previous to the effusion, and the symptoms accompanying the fatal type of the malady may be precisely similar to those induced by effusion. This arises from the vascular turgescence producing an arrestation of the sensorial powers. Even intense irritation of the encephalon may create the same disastrous event, by the same mode of agency. Serres and Cheyne are both very positive that the effusions of blood and water in the brain are but the effects of a morbid action, and that such effusions, per se, do not produce death. Morgagni, Baillie, Abernethy and Cooke, as well as Serres and Cheyne, have given instances in which patients have lived some time after the existence of such effusions. Cheyne thinks that death would take place much earlier in hydrocephalus if the effusion did not occur; for, he argues, that the fluid diminishes the inflammatory action, and imparts a necessary support to the brain. The experiments of Serres go to demonstrate the fact, that in apoplexy it is not the effusion of blood which causes the fatal event, but the phlogosed state of the meninges or brain. My limits forbid any further discussion of this controverted point, as well as that of the compressibility or incompressibility of the substance of the brain. Cooke in his able production on Nervous Diseases has appropriated several pages to the examination of the above points: to him the reader is referred for a candid explication of Serres' views. The Philadelphia Medical and Physical Journal contains Serres' experiments and his deductions in full.

In giving general views of the treatment of a disease, there may be a great illusion created in the mind of the reader, as regards the ready feasibility of its cure. We lay down fundamental propositions, perhaps, in themselves founded on truth; but though abstractly true, yet in the application of them much error may arise. No one truth stands isolated—there is no such thing in medicine as a universality, *a parte rei;* there is

no all comprehending and all absorbing truth, which, like Aaron's serpent, swallows up all other truths. Abstract medical truths entering into the practice of the art, like rays of light penetrating a dense medium, are refracted from their straight line. In the complex machinery of the animal economy; in the multiplied tissues which compose the organism with their diversified modifications of the few simple laws which govern the vital actions; and in the morbid phenomena springing from the various degrees and infinite modes by which disease affects these actions, we have abundant and satisfactory evidence afforded, of the intricacy and involution of those principles which should govern us in our plan of treatment. Were we to contemplate the human system laboring under disease, in one point of view, then a theory built upon abstract principles would be infinitely captivating. But practical skill is the result of comparison and computation—the offspring of a careful observation of facts arranged and systemized, according to the discipline of a sober and severe judgment.

The treatment of hydrocephalus is not to be founded exclusively upon post mortem researches. It must be established on a broader basis,—that of symptoms and pathological anatomy combined. If the plan of cure seemingly contradict our pathology—or if the treatment is coincident with our conceptions of what it should be in such a disease, still we are to be aware of the fallacy which may mislead our judgment in the one case as well as in the other. Thus, in conducting the methodus medendi we are not to reject emetics because in their mode of agency we may consider a manifest opposition exists to the indication of cure required by our view of the nature of the disease. Writers on the Materia Medica and Therapeutics often lay down certain fundamental propositions with a peremptoriness which the facts of the case by no means warrant. To give an instance—the action of an emetic is described as determining to the brain, and whilst its ulterior influence in subduing inflammation about the head, is lost sight of, the remedy is denounced in all cases where inflammatory action exists in the brain, or its meninges. And thus it is that ab-

stract principles delude us in the treatment of diseases. The Egyptian ophthalmy, one of the most virulent forms of inflammatory affection of the conjunctiva, is more tractable under the employment of emetics than all the other means, such as bleeding, purging, and blistering, combined. Here we have a practical refutation of the alleged injury which results from the use of an emetic in inflammations about the head. Hydrocephalus is another, as I shall presently show. We may be misled in our deductions of correct treatment by symptoms, which simulate hydrocephalus. The practitioner must survey the whole ground for himself before he prescribes. When irritation in the alimentary canal, whether proceeding from worms, or ingesta, produce symptoms resembling those of hydrocephalus, the treatment is obvious to every one. When exhaustion, whether induced by catharsis, or a surgical operation, brings on the train of phenomena characteristic of the disease, we must employ stimulants. I doubt the propriety of using opium in such cases except in very small quantities: wine, or brandy, with some nutriment, will be the most appropriate method of restoring the lapsed and rapidly fading powers.

In idiopathic hydrocephalus, where symptoms of inflammation in the arachnoid and pia-matral tissues or cerebral substance present themselves, we must abstract blood; apply cold water, or ice to the head; and purge liberally. Where an affection of the lungs, as bronchitis, either accompanies, from the commencement, the symptoms of cerebral derangement, or is subsequently developed, the emetic treatment is superior to any other; of course under proper subordination to blood-letting when that measure is demanded. It is impossible to say with correctness, whether or not there is inflammation in the brain or membranes in such cases. But as the symptoms indicate inflammation our treatment must be governed accordingly. Dr. Rousseau, who has spoken very favorably of the employment of emetics in the disease under consideration, attributes their beneficial power to the agency exerted by them in correcting the morbid condition of the chylopoietic viscera. But this is a restricted explication, and by no means explains their

entire circle of remedial action. The agency of an emetic in tranquillizing the system, and in carrying off irritation with great rapidity, was long ago noticed, with some surprise, by Sydenham. Irritations of the nervous system, such as neuralgic affections, are often subdued by emetics. Dr. Physick treats neuralgiæ of the face with vomits, successfully. It is in this way the agency of an emetic may remove irritation from the brain, and thus cut short that morbid catenation which, suffered to run on, would mature itself in inflammation. The application of a blister to the head has been much recommended, and as much discountenanced, in the treatment of this malady. I have never seen them do any good, but on the contrary much evil by their excessive irritation, when suffered to remain on the scalp some hours. If applied at all, the back of the neck is a better place than the head.

The bowels are generally constipated in both idiopathic and spmptomatic hydrocephalus. A judicious bleeding will do much to create a susceptibility in them to the action of purgatives. Calomel, aided by jalap, or oil, or senna tea, I generally employ as cathartics. After venesection, repeated according to the symptoms, an emetic will do much in exciting the bowels into action. A mild emetic, such as ipecacuanha, is borne much better by a child than an adult. We perhaps, neglect emesis too frequently in this, and some other diseases of children. Excessive doses of calomel I by no means approve of: what I mean by an excessive dose for a child of six years old, affected with dropsy of the brain, is twenty grains. In those cases where enormous quantities of this remedy are given, the excitability, or organic sensibility and contractility, of the stomach is overpowered, and the purgative effect of the article really hindered by this disproportionateness of dose. A combination of ipecac, with the calomel, is a far better prescription than such Herculean doses. Or the eighth of a grain of tart. emetic may be combined with five or ten grains of calomel. This union will effect more than calomel alone in this disease. I am aware that cases of apparent hopelessness have been recovered by an extraordinary exhibition of calomel, and the use of

great quantities of mercurial ointment. But these were cases of a forlorn hope, and of course extraordinary measures had to be employed. But in conducting the treatment of ordinary cases, we should give precedence to the lancet; next, cupping; —leeching being out of the question in the western states—followed by ice to the head; then emetics; succeeded by purgatives; and in the last place the mercurial plan may be pushed when the other means fail in arresting the march of the disease. Let the practitioner ever keep in mind that he may make a fatal mistake in taking exhaustion for hydrocephalus; the previous symptoms, mode of seizure, plan of treatment pursued, and particularly the effect of remedies upon the case, must all contribute to lighten the path of his judgment, and prevent him from committing ruinous errors in practice.

ART. II.—*Cases of Poisoning with the Datura Stramonium, or Jamestown Weed.* By JOHN SLAVENS, M. D. of Mountsterling, Kentucky.

TO make known to the public those events and circumstances which transpire under our immediate notice, and which are calculated in their nature, or ultimate results, to benefit the human family in any way, is a duty incumbent on all. This duty becomes the more obligatory, when such occurrences relate to the preservation or destruction of health and life. A train of circumstances of this character, having recently taken place under our notice, in the practice of our profession, we deem it our duty to give a faithful and correct statement of them to the medical public. And although the history of the beginning, continuation and termination of these events, go to develope a series of the most cool, deliberate and systematic operations of a solitary individual to destroy the health and lives of a whole family, yet the physician may be able to glean from them some important practical hints in regard to the deleteri-

ous effects of the Datura Stramonium or Jamestown weed, one among the most poisonous substances in the vegetable kingdom; as well as some of the means to relieve the system from these effects. We design to give a history of the cases as they occurred, and the treatment adopted by us, together with such reflections as are deemed necessary, to a correct understanding and clear elucidation of each case. Before entering into a more particular account of each, we will remark that the two first cases were pronounced by us on first sight, to be the effects of the stramonium seed; but it was believed that the children had eaten the poison while at play, as it grew in great abundance about the streets and commons. These two cases were much more violent in their symptoms than any of the subsequent ones, and were the only cases in which any seed could be found in the evacuations from the stomach and bowels.

Owing to the foregoing circumstances, in connexion with the peculiar condition of the subjects of the other cases, we did not at first recognise them to be the effects of poison; for they were labouring under an inflammatory disease, which rendered it entirely impracticable for some of them, and equally improbable that any of them could have taken the poison themselves, either through design or accident; nor could the idea of their being poisoned intentionally by any person, be admitted, as they were a family living in entire peace with all their neighbours; and were not in possession of any ill-natured servants. Nor was it till near four months after, that the truth was unfolded, by a casual occurrence, which led to a train of evidence which very conclusively shewed, that there lived under the roof of this afflicted family an individual who had, during this long time, been endeavouring by frequent, but perhaps small quantities of the stramonium seed, to destroy the health and lives of every member of the family.

On the 23d. of August 1830, at 4 o'clock P. M. I was called in haste to visit E. J. four years old, the daughter of the Rev. Mr. J. of this place. On going into the room her father informed me, with great concern, that his little daughter had gone deranged. I found her in a state of wild delirium, ex-

hibited by alternate fits of laughing, crying, singing and whistling, together with many other strange sounds of her voice; an anxious look, or rather a wild and furious expression of countenance; the eyes were of a scarlet redness, pupils considerably dilated, and a continual rolling of the balls in the sockets. On close examination she was found to be perfectly blind. She would seize with great violence every person with whom she was brought into contact, and every object that touched her; and would very frequently tear her own clothes, and bite and pinch her person. Her whole system was continually in motion, attended with frequent convulsive and spasmodic paroxysms so violent that it required the aid of two or three persons to keep her in any tolerable state of quietness. Her skin was exceedingly hot and dry; and the whole surface of the body was of an erisypelatous redness. The pulse was rather quick and wirey; but neither very tense nor strong.

I enquired whether she had eaten any improper food through the course of the day; or if she had received any injury on the head. Both of these interrogatories were answered in the negative. Her mother remarked, that the child had played about the yard in the forenoon with her little brothers, had eat a light dinner and then went to sleep, and had continued to sleep for some two or three hours, when she awoke in her present condition.

My mind was at once strongly impressed with the belief, that the peculiar delirium, fever, &c. which existed in the present case, were not of that description which is generally consequent upon any of the common causes of irritation in the alimentary canal; or from fatigue or exposure to the vicissitudes of weather. Under this view of the subject, I did not hesitate to express the opinion, that the child had taken some poisonous substance into her stomach, and immediately enquired if the child could have had access to any thing of the kind; but it was thought she could not. I however, insisted that it must have been the case, and suggested the stramonium seed as the most probable substance; and that I should at all events predicate my practice on this supposition.

Owing to the difficulty of keeping my little patient still, I declined the use of the lancet, and determined to administer such medicines as were most likely to produce a speedy and thorough evacuation of the contents of the stomach and bowels; and at the same time to relax the system by means of the warm bath, &c.

Commencing with the use of antimonial wine, I gave half an ounce every ten minutes, till three or four portions were given, without producing the least apparent sickness at stomach, although the warm bath was used at the same time. Finding this course ineffectual, I gave twenty grains of calomel, following it with large draughts of warm water and ipecac; hoping to bring on puking and purging at the same time, but in this too, I was disappointed.

Every symptom of the disease now increasing, I resolved to increase the quantitty of the medicines, and ordered the antimonial wine to be increased to one ounce, and repeated every ten minutes as before; at the same time enemata of a solution of tartar emetic and sulphate of magnesia to be frequently administered. This course was continued until six ounces of antimonial wine were given, and some four or five enemata administered, aided by large and frequent draughts of warm water and ipecac, before puking was excited; and not then till the stomach had become perfectly distended, and the throat and fauces frequently irritated by the introduction of the finger and a feather. On puking she threw up a large quantity of the stramonium seed—say a teaspoonful. The puking was attended with some little abatement of the violence of the symptoms; but after puking some four or five times, the stomach again became torpid, with an increase of all the symptoms of the disease, which seemed to threaten immediate dissolution. In this extreme case of anxiety and danger, I determined to use alternately large quantities of oleum ricini, and an infusion of senna and sulphate of magnesia, aided by frequent enemata of tartar, &c. until the bowels were moved, or my little patient expired. I continued to give these medicines for some three or four hours before the bowels were moved,

and then but slightly, bringing away a considerable quantity of the stramonium seed. During this time I am confident, that I did not give less than six ounces of castor oil, four or five ounces of Epsom salts, and two of senna leaves. In addition, I used upwards of a fourth of an ounce of tartar in enemata. The medicines having operated once pretty freely, and it now being 12 o'clock at night, I discontinued all but the castor oil, a table-spoonful of which I directed to be given every two hours, till free purging was produced. With this instruction I left my little patient for the night.

On my return in the morning I had the pleasure to find her greatly relieved. The medicine had purged her three or four times copiously; the delirium had subsided; fever abated, and there was but little exhaustion of her bodily strength. Every evacuation from the bowels, had carried off more or less of the seed. From this time, she had every evening for a week a considerable fever, attended with slight delirium, and some diminution of sight; all of which, however, yielded to a course of aperients and low living. The quantity of medicines given in the above case may, to many persons, appear extravagant: nevertheless, they were given, as above stated, as many respectable persons can testify.

A few days after the recovery of this little girl, one of her brothers aged six years, was attacked in a similar way, from the same poisonous substance; except that the symptoms were not so violent. I adopted much the same treatment, which was attended with the same happy result.

No other case of poisoning took place in the family till the 13th of October, which was that of Mrs. J. mother of the two children, whose cases have been given. She was taken in labour on the 11th of October: had an easy time and safe delivery. On the 13th I was called to see her. She had a considerable fever, hot and dry skin; some redness of the eyes; a little confusion of mind, though not amounting to delirium; slight pain in the abdomen; and a suppression of the lochial discharge: the pulse was hard and frequent; bowels constipated. I drew sixteen ounces of blood, from the arm—gave ten grains of calo-

mel and four of aloes; directed the feet to be bathed in warm water; and warm applications to the abdomen. Six hours passed and no evacuation from the bowels having occurred, I ordered on ounce of castor oil. 14th. Fever a little abated; pulse quick but neither very hard nor full; no motion from the bowels. I ordered an infusion of senna and sulphate of magnesia, in large quantities every three hours until the bowels were moved: two portions were given without effect: directed a purgative injection, which produced two or three evacuations. 15th. Slight fever, quick, and pretty hard pulse; skin dry and hot; some pain in the region of the womb; no return of the lochial discharge; wild delirium manifested by incoherent and light conversation, with frequent spells of loud laughter; great absence of mind: occasionally, however, she would make some shrewd and keen remarks. I took a few ounces of blood, gave twenty grains of calomel, five of aloes, three of ipecac and three of camphor, to be followed with a strong decoction of senna and polygala senega every three hours; warm applications to be re-applied. In six hours saw her; no evacuation from the bowels; skin a little moist; pulse softer; mind more composed. Ordered an ounce of oil; decoction continued; applied a blister to the back of the neck.

16th. Medicine had not operated; all the symptoms milder. Gave an ounce and a half of oil; and ordered an injection. In four or five hours I saw her in consultation with my friend Dr. Combs: the medicines had operated two or three times freely; delirium relieved; pulse soft; but little fever; ordered the spiritus mindereri.

17th. Had passed a comfortable night; free from fever and had every appearance of returning health. We now directed the bowels to be kept open with mild medicines, attention to diet, &c., which in a few days restored her so as to be able to walk about the house. She continued to improve in her health for a week or ten days, and spoke of riding out; but in this she was disappointed, for she was again taken violently ill with an increase of all the symptoms, from which she had just recovered. I was again called in. Her delirium was of the most

wild and furious character; eyes red; pupils dilated; skin hot and dry; tongue clean; pulse quick, but neither tense nor full; great restlessness. I adopted the usual course of treatment in such cases, and much the same that had been used in her previous attack. After pursuing a depleting course for about a week, she was again relieved of these symptoms; but greatly debilitated: tonics and stimulants were prescribed, and continued with every evidence of their good effects. But as soon as she was able to sit up, and walk about the room, she was again attacked as before. Medicine was again administered, and she once more restored; yet strange as it may appear, she was a third time attacked in a similar way.

It would swell this paper to too great an extent, and perhaps would be uninteresting to our readers to give in detail all the sufferings of this unfortunate and afflicted lady. Suffice it to say, that from the 13th of October till the 17th of February following, (the day on which the person was detected in giving the poison,) she had one attack after another. During this time, Dr. Combs frequently saw her with myself, and every means that we were in possession of, either from our experience in practice, from reading the most approved authors, or from our own ideas of the pathology of the case, was resorted to with but little effect. Frequently we would seem to overcome the disease; but as frequently would it again return.

Shortly after Mrs. J's. confinement, all the children (five in number) were taken with the hooping cough. From this time to the day the detection was made, they all had frequent attacks of delirium, attended with impaired vision, &c.; in short all the symptoms assumed very much the character of those of the two first cases; but were not near so violent, being sometimes so slight as scarcely to be perceived. No seeds were ever discovered in the evacuations, although frequently and carefully examined. The weather being cold and the children sick, they were kept pretty close in the house; and consequently had no opportunity to get any poisonous substance, unless it was given to them by some person, an idea that could not be admitted. These reasons influenced us, to attribute the particular symp-

toms which obtained in these cases, to the inflammatory character of the hooping cough. Our treatment was antiphlogistic. They all finally recovered but the infant, and that I am inclined to believe died from the effects of the hooping cough.

The Rev. Mr. J. being subject to frequent attacks of what is termed in popular language, "the sick headach," frequently remarked to me, that his attacks had become much more severe than formerly, and that his eyesight was so much affected that he could scarcely make an entry in his day-book when he was labouring under these attacks. This however, was accounted for from the loss of sleep, fatigue of body, and great solicitude of mind, consequent upon his assiduous, and unremitting attention both by day and night, to his afflicted family.

The next case which occurred, was that of Miss S. L. sister to Mrs. J. who was on a visit to the family. She was attacked with sickness at stomach, prostration of strength and animal life, almost amounting to syncope. We prescribed stimulants and restoratives under an impression that it was nervous irritation. In two hours from her seisure reaction had taken place; fever high; pulse hard and full; pupils a little dilated; slight delirium. Bled freely; ordered calomel with other active purgatives, which had to be again and again repeated in large quantities, before the bowels were moved. It is perhaps unnecessary to give the treatment in detail; we will, however, remark, that we had recourse to bleeding, purging, puking, sweating and blistering, to a much greater extent than we had been accustomed to, before we were able to arrest the disease. She gradually recovered; but in a short time was taken ill again, much in the same way. The same remedies were used, and were attended with the same success.

On the 17th of February, a casual occurrence took place which gave a clue to a chain of circumstances that unfolded the whole matter, and affixed guilt upon an individual, who of all others was the last to be even suspected, much less to have done the deed. This person gave a portion of the poison to a negro girl who had been instrumental in bringing to light some of the circumstances which led to detection. This girl stated,

that the accused gave her a cup of something to drink, at night; stating to her that Mr. J. had sent it to her to cure her of a cold; but that it tasted so bad that she only took a part of it. On the next morning she was affected with all the symptoms that characterised the two first cases. Recourse was immediately had to medicine, and not until two weeks was she perfectly well.

In closing the history of the preceding cases, I will remark, that in upwards of thirteen years pretty extensive practice, I never witnessed so great an amount of torpor and insensibility of the stomach and bowels, as I did in these cases; nor did I ever before give as much medicine, under any thing like the same circumstances.

It appears in the history of the circumstances, that no seeds were given in substance after the two first cases; but that they were beat in a piece of coarse linen, and then tied up in a cloth, and boiled or drawn in a tin cup; and this decoction was given in various drinks. It is equally clear that this poison was administered when the members of the family were most indisposed, no doubt with the view that the effects should be supposed to result from their present diseases; and in order the the more effectually to produce this impression, it was given in small quantities at a time, lest if large doses were given, the symptoms should at once be recognised which might possibly lead to detection. Having a knowledge of the quantity given in the two first cases, it was an easy matter to graduate the doses in the other cases, so as not to produce such effects as would be readily recognised as cases of poison. In the case of the negro girl it was evidently the design to produce immediate death, which would probably have been effected had she taken all that was presented to her.

The foregoing cases go most clearly to confirm two great practical principles in pathology and therapeutics, that should never be lost sight of in our treatment of diseases. First, the propriety and necessity of giving medicine in proportionate quantities to the amount of disease. Second, that there is no danger to be apprehended from large doses of medicine, pro-

ided we select the appropriate remedies to the case, and in
ne administration of them we do not too far over-reach the
mount of disease. We are, perhaps, all prepared to admit,
hat to remove any disease we must use agents calculated to
nake a stronger impression on the system than the diseased ac-
ion; otherwise the disease will maintain its ground. By a re-
erence to the history of the first case, these principles will be
ound to be most clearly established. Had we in that case giv-
n medicine in the usual doses we should certainly have lost
ur patient. And on the other hand, if we were to give the
ame amount in an ordinary case of disease, that we did in this,
e should most probably lose our patient. How important
hen to have clear and correct views of every case we are call-
d to see, both as it relates to its peculiar kind and the extent of
norbid excitement. Without this kind of knowledge our prac-
ice will in general be weak and empirical—consequently un-
uccessful.

Another very important matter exhibited in the character of
he preceding cases is, the practicability of giving small and
requent quantities of the stramonium without being able to
letect it, so as gradually to destroy health, especially when
he persons to whom it is given, are labouring under the influ-
nce of disease, as was the fact in relation to the cases just re-
orted. This becomes the more easy to be effected, when the
articular condition of the person to whom it is given, is such
s to render the production of a disease probable, between the
ymptoms of which, and the effects of the stramonium, there is
striking similarity, as was the case of Mrs. J.; for we suppos-
d her disease to be that of *puerperal mania.*

May it not be probable that in many cases of fever &c. espe-
ially among the coloured population of our country, this or
ome other poison may be given in small or frequent portions,
o as gradually and almost imperceptibly to destroy the per-
ons to whom it is given. We are aware of the prevailing and
trong prejudices, of this degraded part of society in favour
f the idea of poison: And we are well aware of the set-
led and determined opposition of physicians generally to this

idea. But may it not be probable that we treat with too much neglect, the popular opinions of even this ignorant and unfortunate part of mankind? We should ever remember, that general and popular opinions among any people, are seldom without some foundation in truth. What an admonition should this be to every practitioner of medicine, to be very minute and particular in his inquiries into all the probable and even possible causes of disease, whether remote, predisposing, exciting or proximate; and when he is not clear in his mind, as to the real cause, he should with an untiring zeal prosecute his inquiries until he is made acquainted with some fact, that will unfold the whole matter. And if the preceding cases and reflections shall produce this spirit of inquiry in the mind of any one practitioner, so as to be the means of saving the life of one solitary individual, the writer of this rough and unpolished paper, will be more than compensated for his trouble.

ART. II.—*Thoughts on the Original Locality of Fever.* By CHARLES CALDWELL, M. D.

[READ BEFORE THE LEXINGTON MEDICAL SOCIETY, JANUARY 1830, AND PUBLISHED BY REQUEST.]

I HAVE elsewhere* stated in substance, and now repeat, that the views I am about to deliver on this subject, whether right or wrong, are my own, and were not derived by me from the writings of any other physician, living or dead. I had entertained them, and, under the charge of an undue devotedness to "professional speculation," often contended for their truth, long before Broussais was known in this country as a writer, before the name of Clutterbuck or Prost had been heard of by me, and before I had looked into the writings of Van Helmont, Baglivi, Bartholin, Lindamus, Bordeu, or Reda, or had the slightest knowledge of their opinions on fever. While

* Vid. Analysis of Fever.

I was yet a student of medicine, or soon after my graduation, I read a paper on the "original locality" of fever, in the Philadelphia Medical Society, which I doubt not is preserved in the archives of that institution. About the year 1804, moreover, a gentleman, then a pupil, but now a physician of high standing, in the State of Kentucky, read before the same body a dissertation intended to show, that bilious fever results from a *primary deleterious affection of the stomach;* in other words, that it is a disease of *gastric origin.* The subject was suggested to him by myself, and the article prepared in part from materials with which I furnished him. Were I to request it, I could procure from him a statement to this effect.

If I am indebted for the first hint of the original locality of fever to any author, it is to Dr. Darwin, whose Zoonomia I read at an early period of my medical studies. Yet I have no recollection of having received it from him; nor is he so definite on the subject as to induce me to believe, either that he himself distinctly understood it; or that I derived from him any precise and leading suggestion in relation to it. He certainly pronounced fever to be a disease of "Association," in which his meaning must have been, that it begins in one part of the body, and travels to the others, in obedience to established constitutional laws; and that it does not attack every part at once, constituting from the beginning an "affection of the whole system." But, to make the best of them, his views and expositions on this point are tangled and obscure, and do not possess sufficient explicitness to persuade me now, that I was ever, at any period, profitably instructed by them. Besides, when he even states an interesting fact, or a valuable truth of any kind, he usually so involves it in some fancy of his own, as to render it useless, if he does not pervert it to a wrong purpose. Add to these the further consideration, that my views of fever differ so widely from those of Dr. Darwin, as to forbid the belief that they were derived from him. But whatever might have been its origin, my opinion on this subject was formed, as already mentioned, at an early period in my professional life, and, though modified in some points, as experience and further

reflection advised, has never been changed in its fundamental principles.

Notwithstanding its antiquity and extensive prevalence, and the high authority of many of its advocates, the belief in the existence of *idiopathic fever*—I mean fever *sine ullo morbo locali*—not accompanied by any *local* affection, has long appeared to me to be one of the most unfounded and improbable in the science of pathology. And the more strictly I scrutinize it, the deeper is my surprise, that it still has defenders among enlightened physicians. I am unable to discover the slightest foundation for it either in fact or analogy. Every principle of living organized matter, and every law determining its action appear to me to be in such direct opposition to it, as to give it the character of an obsolete superstition. When I shall be able to find, or rationally conceive of, an oak tree growing and flourishing without a root, a human being regularly supplied with wholesome chyle without the aid of a digestive apparatus, a stream constantly flowing without a fountain of water to feed it, or any other effect occurring uniformly without a cause, then may I be induced to believe in the *possibility* of a fever existing independently of a topical læsion; not before. The hypothesis bears the same character and belongs to the same class of abstractions with that of the existence of mental derangement without any corresponding affection of the brain. Each vision presents itself as an effect without a cause, and, like every other vision, vanishes under the influence of genuine light. An idiopathic fever is a professional mystery, which affords no basis for sound belief to rest on. It must be either rejected, therefore, or embraced in a spirit of professional credulity. But I need scarcely add, that such a spirit cannot long sustain itself under the keen analysis and uncompromising common sense which mark, at the present period, inquiries in medicine, as well as in most other branches of science. Instead of a belief in medicine, resting only on presumptive grounds, the science and the times call for a belief founded on competent and positive evidence. Every thing short of this is regarded as hypothesis, and denied a place in professional knowledge.

In the minds of those (and their number is great) who do not strictly examine what is communicated to them orally and in books, but receive it under its popular meaning, words and phrases loosely employed implant much error. This is in a remarkable degree the case, as relates to the expression, which is in as general use as any in medicine, "Fever is a disease of the *whole system*." In common conversation about the minor affairs of the profession, the idea conveyed by this expression may pass freely and do but little harm. But it ought not to be admitted into grave pathological discussion, because it is not strictly true. The words carry the mind of the unwary far beyond the fact. The phrase is much too strong for the occasion, and misleads those who implicitly believe it.

Fever is so far a disease of the whole system, that, during its prevalence, especially if it be severe, none of the soft parts of the body perform their functions as well as they do in health. Yet, accurately speaking, some of them can scarcely be said to be diseased. Their condition, although not precisely what it ought to be, cannot with propriety be called morbid. In no instance of the kind certainly will an enlightened pathologist allege that every part of the system is diseased alike. Still further will he be from maintaining that they were all equally affected at the commencement of the complaint. In no fever, usually so denominated, are the muscles, tendons, ligaments, skin, glands, and lymphatics in a state of as deep and dangerous læsion, as some portions of the abdominal viscera. As relates to the lungs, the same is generally true. Nor do they suffer at so early a period of the complaint. Under many attacks of fever, vision, hearing, and feeling are sound, and respiration but slightly deranged. Why? because the organs of those external senses are entirely uninjured, and the lungs nearly so. And although the muscles are enfeebled, and the glands disordered in their secretory action, it is by sympathy, not in consequence of a primary injury. The same may be affirmed of many other parts of the body, which it would be superfluous to mention. As to the bones and cartilages, and even the parietes of the heart and blood vessels, it is doubtful whether, in

most febrile cases, they are affected at all. I mean in their own substance and texture. All known facts which bear directly on the subject, favor the belief that they are not. The *action* of the heart is indeed disturbed. But that arises sympathetically from the derangement of other parts. Except in fevers of a peculiar character, or a very high and dangerous grade, that organ exhibits no evidence of disease in itself. It is not marked by any læsion, which could be the cause of its deviation from healthy action. The reason of all this is plain. A morbid condition of the organs referred to is not essential to the production of febrile affections.

But as relates to certain other organs of the body a very different state of things prevails. No case of fever, called idiopathic, has ever been witnessed, without some derangement of the abdominal viscera. Were I to say that, without such derangement, fever cannot exist, facts are not wanting to sustain the assertion. If those parts are not marked by tenderness perceptible on pressure, which is much more frequently the case than is generally imagined, the disease is at least accompanied by flatulency, defective, excessive, or perverted appetite, nausea, vomiting, an unusual fulness or a sense of heat in the abdomen, costiveness, diarrhœa, vitiated secretion, a red and foul tongue, or some other abnormal phenomenon indicating a diseased condition of the stomach or bowels. And the removal of such condition removes the complaint. Should the malady however prove fatal, *post mortem* examination leaves the matter no longer dubtful, even with those who had been previously unbelievers. Let the examination be strict and skilfully conducted, and either in the brain or the viscera of the thorax or abdomen, more especially the latter, some topical læsion is universally found.

On this statement alone, the truth of which, no physician of experience will deny, I might rest the position, that fever is universally accompanied by a topical affection. For, strictly speaking, to constitute such affection, nothing is requisite but that some one organ or part of the system should be more deeply diseased than any other, and that its affection should

contribute to the derangement of others. When the eye is inflamed there exists a topical læsion. Why? Because that organ is more diseased than others. When the skin is burnt or lacerated, the same is true, for the same reason. When a bone is fractured or a muscle divided by a cutting instrument, when any organ has sustained a contusion, or when venous congestion exists in the brain or lungs, the presence of a topical læsion is acknowledged. The reason is plain. The parts specified are more diseased than the other portions of the system, and are instrumental in the production of further disease, through the agency of constitutional laws. Why then is not the same acknowledgment made, when the chief derangement is in the viscera of the abdomen? That it ought to be made cannot be even questioned, much less gainsaid. No one will deny that a diseased condition of the stomach or bowels, other parts of the body being less deranged, is as much a local affection, as a diseased condition of the lungs or brain, or even as the fracture of a bone, or the rupture of a blood-vessel. The case is greatly strengthened by the consideration, that, from such condition of the organs referred to great mischief necessarily results to the entire system.

But this view of the subject, although incontrovertible, constitutes neither the whole, nor even the most conclusive portion of the evidence that may be adduced, in favour of the doctrine which maintains the original locality of fever. I employ the phrase "*original locality*" in its most literal meaning. My opinion is, that fever is not only always and necessarily accompanied by a local affection, but that it arises from it as its immediate cause. And this mode of production I believe to be as indispensible, as that the plant should spring from its parent seed, or the flower from the stalk that bears it. Without a topical affection, as its origin, fever cannot have being. Of this affection, all the phenomena denominated febrile are but symptoms. Every form of fever therefore is strictly symptomatic; that produced by marsh miasmata as positively so, as that resulting from a gunshot wound. Every essential and characteristic symptom of fever, that can be fairly interpre-

ted, points to some local injury, as its productive cause. Let this subject be more minutely examined.

What are the most essential and characteristic symptoms of fevers called idiopathic? in a special manner, what are their earliest symptoms? These questions have been, in part, already answered. The symptoms inquired after are manifestations indicating some kind of abdominal distress or uneasiness; I should rather say that they themselves constitute it. In that distress, any morbid feelings that might exist in other parts of the body, are, in a great measure, merged. The principal and most painful of these are generally in the head and back, and sometimes, though less frequently, in the thorax; and they are usually sympathetic and secondary; mere branches from a distant root; and that root is a local affection of the chylopoetic viscera. This truth is sustained by the fact, that a removal of the chylopoetic derangement restores soundness to the head and back, while remedies addressed immediately to those parts themselves do not. Let those who may be opposed to me in opinion adduce a single case of fever, in which every part of the system is alike deranged, either really or apparently, and I will surrender the argument; and if they cannot, it might be discreet in them to surrender it themselves, before it be wrested from them. In the examination of the bodies of those who have died of fever, has a single case ever been found, in which topical læsions did not exist? I answer confidently, no; and medical records sustain me in the reply; as far, at least, as a negative can be sustained. But my analysis must be more circumstantial.

That fever commences in a local affection, and becomes general, as far as it does become so, by association or sympathy, appears from a variety of considerations, some of which are the following. The causes productive of it can act only locally, not generally; the earliest symptoms of it point distinctly to a topical læsion, and all the subsequent ones do the same; the most successful mode of treating it indicates its local origin and seat; every febrile disease, whose origin appears from ocular evidence, can be traced with certainty to a topical affec-

tion; and, as heretofore suggested, *post mortem* examination establishes the doctrine by testimony as conclusive, as any connected with medical science. In illustration of each of these sources of proof I shall offer a few remarks.

The fevers denominated idiopathic, and pronounced by writers on nosology to be unaccompanied by a topical affection, are pestis vera, yellow fever, and all other forms and grades of that produced by marsh miasmata, and typhus fever. Scarlatina, measles, and the entire family of eruptive fevers are, I think, excluded from the class, as being evidently connected with local affections. So is fever from improper ingesta.

The causes then productive of the fevers under consideration are malaria, humidity, and vicissitudes in the atmosphere; I mean in its sensible qualities. No others can be adduced. Indeed it might be safely affirmed that malaria, under some of its modifications, is itself the only true remote cause, moisture and atmospherical changes being only exciting causes. But, admitting them all to stand on the same footing, the primary action of these is local, and cannot, in the nature of things, be otherwise. No cause can act primitively where it does not formally and positively exist. In simpler terms; no agent, deleterious or salutary, can produce a primary effect on any part of the body, with which it does not come into immediate contact. This must be received as an axiom in physiology. But there are only three tissues or parts of the body, with which malaria, moisture and vicissitudes in the sensible qualities of the atmosphere can come into contact. These are, the skin, the mucous membrane lining the nares, trachea, and bronchiæ, and that which lines the fauces and alimentary canal. They cannot reach immediately the entire nervous and cerebral system, the heart and bloood-vessels, the absorbents, the muscles, the glands, or any other part but those specified. On them therefore they must primitively act. And they act on them as organized parts, composed of their several elementary tissues; not on any one of those tissues exclusively. They do not act on nerves alone, blood-vessels alone, or absorbents alone. They act on the entire part, consisting of nerves, blood-vessels, absorbents, and

cellular membrane. No matter whether the nervous be the more susceptible and important tissue or not; the others are equally essential to the result. Let either of them be destroyed or deeply injured, or its character in any way materially changed, and the susceptibility and powers of the part will be changed.

The effect thus produced by the causes in question must be necessarily local. Suppose them to injure the entire skin, the læsion is topical, because that tissue constitutes but a small portion of the body. Let the part injured be the mucous membrane, extending from the external nares to every air-cell of the lungs, the affection is still topical for the same reason. And the same is true, should the læsion embrace every portion of the mucous lining of the alimentary canal, *ab ore ad anum*. In no case is it possible, then, for these causes to produce a *general primitive* disease. Yet they do produce the complaint denominated idiopathic fever. They must do it, therefore, by a local affection, which becomes general through the medium of sympathy. Like every other noxious agent, they injure first the parts they touch; and the influence of the deleterious impression thus made spreads and becomes general, in obedience to laws which so govern the constitution, as to render the system a living unit.

I know it is contended that the febrile miasm makes its way into the blood, contaminates it, circulates with it, and thus produces general mischief. This pathology, to say the least of it, is purely conjectural. Nor, even as a conjecture, does it possess, in itself, any thing to recommend it and give it weight. But it has advocates in physicians worthy of a better cause, and must therefore be examined.

There are but three routes, by which the miasm can make its way to the blood; the lungs, the skin, and the alimentary canal. In entering by the two last, its passage is long, as it must travel necessarily by the course of the absorbents. The hypothesis of "venous absorption" either from the inside of the stomach, or the outside of the skin, does not appear to be founded in truth. Were this a suitable occasion to examine it, ob-

jections to it, deemed unanswerable, are not wanting. The office of veins, strictly so called, is to take up and convey blood. But, in a healthy condition of the body, no blood is deposited either within the stomach, or without the cuticle. The existence of veins, therefore, opening on either surface would be superfluous. But nature forms only parts that are necessary and fitting in her works. She creates nothing for mere show, but only things that are practically useful; and these are never permitted to be idle. In entering the blood by the lungs, the route of the miasm is much shorter. But in effecting its passage either way, it must first come, as already mentioned, into contact with solid matter; with the skin and absorbents, by one route; the mucous lining of the alimentary canal and the lacteals or common absorbents, or both, by another; and the mucous lining of the lungs, with other portions of that viscus, by the third. But, consistently with any known law of the animal economy, is it possible for a poison of a formidable character to pass through those highly sensitive and irritable organs, without injuring them, mingle with the blood, and deeply, often incurably injure them and other parts afterwards? When it first comes, fresh and undiluted, into contact with the skin, lungs, and alimentary canal, are not its fitness and opportunity to produce mischief much better than it is subsequently, if mixed with the entire mass of blood, and thus necessarily diluted and weakened? To the latter question, no one, I think, will give any but an affirmative, nor to the former, but a negative reply. To answer otherwise, in either case, would seem to be a violation of common sense, in behalf of a belief which is no better than a paradox, and cannot be maintained. Admitting that the poison really enters the blood, it must first injure the part through which it passes, and produce there a local affection. This seems a necessary result. The contrary opinion is opposed by every fact ascertained by observation, and every inference deduced by reason. It would be about as rational to contend, that a handful of mustard-seed shot, issuing from a fowling-piece, could penetrate the skin, lungs, or alimentary canal, without injuring them, min-

gle with the blood, and then destroy life, by acting on that fluid, as to assert the same of any kind of malaria.

But what tenable ground have the humoralists for maintaining that febrile miasm finds its way into the blood at all? They will not affirm that they have detected it there, because the contrary is known to every one, and acknowledged by themselves. Nor will they allege that by inoculating with the blood of a febrile patient, or injecting it into the veins of a person in health, they can communicate the disease. The contrary of this also is known, although the notion once made an article in the humoral creed. Experiment satisfactorily proves, that even the blood of a person labouring under small-pox cannot, by inoculation, propagate that complaint. But I need scarcely add that, were the blood contaminated by a febrile poison, the reverse would be true. In that case the disease might be conveyed by it from the sick to the well, as readily as by inoculation with variolous matter extracted from a pustule.

As far as I am informed on the subject, the humoralists assign but two reasons for their belief in the contamination of the blood in idiopathic fever; *its dark colour*, and its *incoagulable condition*. In as much, say they, as the blood, in fever, is neither so red, nor so coagulable as it is in health, it holds in mixture, as the cause of both, a portion of the poison productive of the disease. Such is their reasoning; and it cannot be rendered more inconclusive. It is loose and vulnerable in every point. They have as much and as fair ground to say, that the blood is dark coloured and incoagulable, because it *does not* hold miasm in mixture, as because it *does*. On what evidence do they contend that miasm blackens the blood, by being mingled with it? Have they ever verified their supposition by experiment? They will not reply affirmatively. To make the most and best of it then, their belief to that effect is mere conjecture; although they utter it as confidently, as if it were a demonstrated truth. Let its soundness be tried by an examination of the phenomena of intermitting and remitting fever.

If the miasm, which produces those complaints, be in the blood at all, it must be in it constantly from their beginning to their

ond; at least from the commencement to the close of the same paroxysm. And being thus present in it, it must darken it alike at every stage of the complaint. But how does this inference comport with fact? Is the blood equally dark at the beginning, middle, and close of each paroxysm, and in every stage of intermitting and remitting fever? We know it is not. In common cases it is dark only during the access and cold stage, becoming, during the exacerbation or stage of excitement, as florid as it is in health. It sometimes becomes much more so, the venous blood assuming not a little of the arterial hue. As well might I contend, then, that marsh miasm reddens the blood, as my opponents that it darkens it; in as much as, in the course of the same paroxysm, as will more fully appear hereafter, that fluid is preternaturally red and dark by turns. Every practitioner of experience must have observed, that blood drawn from a patient in bilious fever, when the circulation is rapid and vigorous, is nearly of arterial floridness. He must have also observed that when extracted in deeply congestive cases, where the circulation is languid, and the respiration restricted and oppressed, it is dark at first, but becomes more brightly red, as the pulse rises, the current increases in volume and force, and respiration is rendered more free and full. These facts, I say, must be familiar to every physician of extensive experience and correct observation. And they are subversive of the hypothesis, that marsh miasm darkens the blood by becoming mixed with it. Nor can the advocates of that notion adduce, in support of it, an authentic fact of any description. It is purely conjectural; no better (and it cannot be worse) than another kindred vision, broached by chemists on the same subject; that the dark colour of venous blood results from the admixture of a superabundant portion of carbon or carbonic acid gas with it. The fancy entertained by some, that such admixture is the cause of autumnal fever, cannot be surpassed in error. It is not sustained by a shade of probability, much less by a single fact.

The darkness of the blood, observed occasionally in bilious and other fevers may be easily explained, in conformity with well known laws of physiology. It is produced generally by

the joint operation of two causes; languid circulation, and defective respiration. Either of them, indeed, can produce it alone. It need hardly be observed, however, that the two causes are for the most part united. When respiration is imperfect, the circulation, for reasons familiar to physiologists, cannot be active and vigorous. The lungs, by *respiration*, arterialize and redden the blood. Weaken or in any way deteriorate that function, and its effects are diminished in a degree corresponding to the injury it has sustained. The consequence is, that the blood is dark. Nor is this the only consequence. The action of the heart declines in vigour, and the circulation grows languid. The blood is out of harmony with that organ, especially its left auricle and ventricle, and does not excite it to the necessary action. To physiologists all this is known to be true. Its cause is also known. To induce them to credit it, therefore, no proof beyond what they already possess is requisite. Its suitableness to throw light on the point I am considering is plain.

During the access and cold stage of intermitting and remitting fever, respiration is imperfect, the heart acts feebly, and the circulation is languid. Of this state of things, as a cause, a dark colour of the blood is a necessary effect. During the stage of exacerbation, things are different. Respiration is restored, the heart has returned to vigorous action, and the circulation is full and strong. Hence the blood becomes florid again. That such is the case, appears conclusively from the state of the complexion, which is now of a much brighter red than in health; whereas, during the cold stage, it was pale and livid. Instead of saying then that the blood is *merely florid*, when the excitement is very high, and the action of the system vigorous, I might have declared it *preternaturally* so. In congestive fever, and in the last stage of every protracted and malignant one, a condition of things exists, as respects the lungs and the circulation, similar to the cold stage of an intermittent. The heart acts feebly and respiration is defective. In each case, therefore, the blood is dark for the same reason. Nor has the mixture of a miasm with it

the slightest influence in either. No such mixture, in fact, exists. This assertion rests on considerations that cannot be overthrown. It has been already stated that no miasm is found in the blood. But this is not all. Draw blood from a person in health, and expose it to an atmosphere, in which that poison abounds, and its colour is not darkened. Blood already dark coloured may be in this way brightened. This experiment I have seen repeated hundreds of times, during the prevalence of bilious epidemics, with the result here specified. But did marsh miasm darken the blood by mixture with it the issue would be different. The fluid would lose its florid die. In malignant fever, with languid circulation, produced by a wound of the stomach, or any other deep læsion of that organ, or by thirst or famine, the blood is dark. Yet it contains no miasm. I once saw and attended a case of fever, the result of a fatal inflammation of the stomach, produced by the accidental swallowing of an excessive dose of corrosive sublimate, in which dark blood was copiously discharged from several parts of the body. Poisoning with arsenic, in which the primæ viæ are the seat of disease, and the fever is symptomatic, is productive of similar phenomena. The blood of animals in a state of hybernation or torpor from cold, is dark. Why? Because it holds a febrile miasm in mixture? No; but because respiration is defective or entirely wanting, and the blood circulates feebly or not at all. But it is needless to run any further into details. Be the cause of the weakened and deteriorated functions of the lungs and heart what it may, let that condition of those organs be produced, and the blood will be darkened, with as much certainty, and as directly in obedience to settled laws, as the heavens are by the setting of the sun.

Slow movement, as heretofore intimated, gives to the blood a darkened hue. That entire stagnation does can be proved at any time. Pass around the arm of a person, in health, a bandage so tightly, as to obstruct the movements of the venous blood. Suffer it to remain in this condition for a few minutes. Open then a vein, and the blood, on first flowing, will be preternaturally dark; but, as the stream continues, its florid and

healthy colour will return. The explanation of this is plain. The dark blood is that which has been stagnant in the veins of the arm, and been deprived for too long a time of the influence of respiration. It is analogous to the blood of a hybernating animal. The florid blood comes from a distant part, where it had not been stagnant.

We are told, on high authority, that, during the prevalence of a bilious epidemic, the blood and alvine evacuations of every one exposed to the malaria are preternaturally dark, whether his health be affected or not. This, as relates to the blood, is certainly a mistake. Under the circumstances here specified that fluid is not darkened in those whose health is unimpaired. I have often examined it, in such a case, and know that I speak correctly. Many others have done the same, and the result of their observation has coincided with that of mine. Nor, when the system is perfectly free from disease, have I ever observed the alvine discharges to be dark. I firmly believe they are not so. The colour of those discharges, even when it is dark, does not depend on the colour of the blood. It arises from the deranged secretory action of some of the abdominal viscera. But, under such derangement, the individual is not in health.

Admitting it to be true that, while a bilious epidemic is prevailing, the evacuations from the bowels, and the complexion of every one within its range are darker coloured than usual, the phenomenon readily admits of solution. Every one is exposed to the action of the miasm, and is more or less deranged in health by it. He is predisposed to the complaint, and requires nothing but an exciting cause to bring it into action. But this predisposition consists in some derangement of the abdominal viscera, especially the liver. Hence arises the discoloration of the stools and complexion. A dark colour of the blood, in consequence of the admixture of febrile miasm with it, is not the cause. The serum of the blood may be yellow; but, if the respiration be healthy, the red globules are not darkened.

Nor has the non-coagulating condition of the blood, in fever.

any more connexion with marsh miasm than its dark colour. That phenomenon also depends on a very different cause. The coagulation, or rather contraction of the blood is a vital process; as much so as the contraction of a muscle, and is strikingly analogous to it. The more vitality the blood possesses, the sooner it contracts after being drawn; and the reverse. Hence arterial blood contracts more rapidly than venous. Destroy completely its vitality, and it will not contract at all. Blood drawn during the cold stage of an intermittent contracts slowly and loosely, because it possesses but little vitality. That drawn at an advanced period of a malignant fever, or toward the close of a protracted one, contracts but little, for the same reason. But that this does not depend on any miasm being mixed with it, appears from a variety of considerations. The phenomenon occurs as readily and certainly in a malignant or congestive fever resulting from a wound or a burn, as in one produced in any other way. Let an individual be so disabled, by a blow or any other mode of concussion, that his respiration is for a time suspended, and his blood forms a very loose coagulum. Extinguish life suddenly and completely, in any way, and the blood will not contract. Is an individual destroyed by lightning? His blood remains fluid. Is he killed by a severe blow on the scrobioulus cordis? The same is true. Is a stag run to death, or a bull baited until he falls dead? In neither case does the blood contract. Yet here is no miasm mixed, or suspected to be mixed with it. It remains fluid from a very different cause. It is completely deprived of its vitality, and is therefore incoagulable. And this is the only reason why it is so in any state, or at any period of fevers produced by miasms. In further proof of the opinion for which I am contending, I may observe, that incoagulable blood is always dark coloured. Hence in deeply congestive fever, where, as already mentioned, the blood, when the vein is first opened, is dark, and, as it continues to flow, becomes more florid, let the dark portion be received into one cup, and the florid into another, the former will remain fluid, and the latter will contract. The reason of this is plain. Owing to impaired respiration, from which process

the blood receives its vitality, the dark blood possesses very little of life; but the lungs being relieved and respiration improved, by means of venesection, the scarlet blood possesses more. But if, by bleeding, respiration be not improved, the blood becomes neither more florid, nor more contractile.

But I have yet another reason for denying that febrile miasm passes into the blood, and in that way produces disease. Were that hypothesis true, the heart and blood-vessels would be the first parts to suffer from it, because the poison would first act on them. Deranged circulation would therefore be the earliest symptom of fever. But I need not say that this is not the case. Every physician of correct observation knows it is not. At the beginning of fever, patients are indisposed and complain for some time, frequently a considerable time, before there is the slightest alteration in the pulse. Nor can regular fever be produced by the direct introduction of any known poison into the blood. Disease can be thus generated, and life destroyed. But, I repeat, that fever, in its regular form, cannot. Certainly in none of the experiments of introducing poisonous matters into the blood of animals that I have ever witnessed (and they have been numerous) did any febrile symptom arise. It will appear hereafter, however, that fever can be produced, by the introduction of a poison into another part of the body, where its first effect is a topical læsion, which is soon succeeded by phenomena, not distinguishable from those that result from the influence of marsh miasmata. Let the subject, then, be viewed and examined as it may, there exists no shadow of evidence that febrile miasms of any description make their way into the blood.

But suppose they did thus enter, even then the doctrine of the original locality of fever would remain unshaken. The heart and blood vessels, I say, would suffer first. This would seem a necessary consequence of their being first exposed to the febrile poison. But they constitute only a part of the system. A læsion produced in them, therefore, even throughout their whole extent, would be but topical, and could reach other parts only by sympathy. Hence a fever thus produced would be as

really symptomatic, as that resulting from a mechanical injury.

I have alleged that, from the beginning to the end of fever, the prominent symptoms steadily point to a local affection. This is true of all the symptoms we are able to interpret; and the detail of it has been already perhaps sufficiently given. Unless the disease comes on so rapidly as to preclude examination, the patient's first perception that he is indisposed, arises from something wrong in feeling, or function, or both, in the chylopoetic organs. Nor does he lose that perception until the complaint is removed. Other symptoms may, and often do, for a time at least, disappear. But that continues. Hence the return of natural appetite, and a natural condition of other abdominal functions and feelings, more especially natural secretions, is the most unequivocal sign of convalescence. From all this the inference seems plain. The original seat of fever is in the viscera of the abdomen, and is of course a local affection.

I know we are told that all cases of fever are not ushered in by a sense of gastric derangement; but that some persons enjoy perfect health and a natural appetite until the complaint fairly developes itself, and even experience an unusual flow of elastic spirits and pleasant feelings just before the attack.

That something like this may occasionally occur, is not denied. Fever does attack at times, with but little warning. But the event, being rare, is a fair exception to the general rule. And it is from the rule, not from exceptions to it, that inferences are to be drawn. Exceptions are but uninterpreted anomalies; and, being deviations from the general course of things, cannot be embraced in general reasoning.

The cases of fever referred to then do not avail my opponents any more than myself. They are as inexplicable on their principles, as on mine. I think more so. We can as easily conceive how the stomach may struggle successfully, for a time, against a febrile miasm, without the brain feeling it, or the other parts of the system suffering, as how that miasm may float for days and weeks and sometimes even months in the blood, and do no perceptible mischief. Indeed a very strong

objection to the hypothesis of the contamination of the blood, arises from the length of time that often intervenes between exposure to a febrile poison, and the attack of the disease. That time I say frequently embraces a period of several weeks. But it is contrary to all analogy, and therefore to probability, that a poison should, for so long a space, float in the blood, without doing any perceptible harm. Introduce poison into the blood, and it acts, not in hours, days, or weeks, but immediately. To this I know of no exception. But a morbid irritation may lie dormant for a considerable time, and then do mischief. Thus a punctured wound often heals up, ceases to be felt or even remembered; and from three to six weeks afterwards, produces tetanus.

It is only the congestive and most malignant forms of fever that attack suddenlyand without warning. The patient sometimes falls down, as in apoplexy, or as if stunned by a severe blow. Or if he does not actually fall, he reels and totters, like a person intoxicated. But these phenomena are most characteristic of an injury done to the stomach or the head. A shock to the stomach produced by a blow or otherwise, stuns, knocks down, and even kills, as readily, as a shock immediately to the brain. But such an injury may be inflicted in various ways. I have seen persons, when greatly heated and fatigued, by exercise in hot weather, destroyed in a few minutes, by a shock given to the stomach, by a draught of cold water. I have witnessed a similar effect from the shock of an inordinate draught of ardent spirits. A sailor in Philadelphia, drank, on a wager, in five minutes, three pints of Holland gin, and expired immediately. Why, then, may not a deep impression, suddenly made on the stomach, by febrile miasm, so shock the system, as to produce immediately an attack of fever? In many instances, individuals exposed suddenly to a large amount of this poison, in a concentrated condition, have sickened on the spot, and been unable to walk. It will not, I think, be contended that, in these cases, the poison entered the blood. It must have operated by a shock, through the medium of the nerves. On no other ground is the suddenness explicable. The impression

producing it therefore was topical. A belief the contrary of this cannot be sustained. But cases of disease thus instantaneously produced are always deeply congestive. The blood in them therefore is dark.

Some oppose to the opinion of the gastric origin of bilious fever an assertion, that the poison cannot find its way into the stomach. This is a singular error, resulting necessarily from a want of attention to a fact which ought to be familiar to every one. Whatevever floats in the atmosphere in a gaseous form cannot fail to be conveyed into that organ. Every one swallows his saliva much more frequently than he is conscious of doing, and, in each act of deglutition, forces into his stomach a quantity of air. The slightest attention to the subject will convince the most sceptical that this is true. We swallow air, moreover, with every mouthful of our food, and with the first portion of all we drink. It is not merely the air which becomes entangled with our food during mastication that passes down the œsophagus along with it. Every bolus or portion of aliment we swallow protrudes before it, in its descent, a quantity of air in a disentangled condition. This is a necessary effect of deglutition. Whatever floats in the atmosphere around us, therefore, in a highly attenuated state, whether it be poisonous or salutary, is in this way brought into contact with the mucous lining of the stomach, and may there act according to its nature. It is believed to be in this manner that metallic exhalations often produce deleterious effects.

The most successful mode of treating fever indicates its local origin. This consists in such measures as are best calculated to remove congestion from the chylopoetic organs. The chief of these is the production of free and natural secretions from those viscera. I have never seen a case of fever that did not yield to such evacuations, when they were fairly produced, skilfully managed, and long enough continued. Nor have I ever seen a case of violence and danger cured without them. Remedies that reduce excessive general excitement succeeded by those of a revulsive character do much, and are indispensible in the treatment of fevers of high action. They are often

necessary to prepare the system for secretory remedies, by reducing it to the secreting point. But they cannot alone effect a cure. Some of the abdominal viscera are in a state of topical congestion, often of inflammation, which can be removed only by healthy and copious abdominal secretion. Produce such secretion, at the commencement of fever, before the complaint has become confirmed, and you arrest it in its course and thus shorten its duration. Autumnal fever is often prevented or greatly curtailed in its period by a free bilious diarrhœa or a cholera morbus, occurring spontaneously or produced by artificial means. I have seen hundreds of patients rescued in this manner from threatened attacks of yellow fever, and other forms of bilious disease. This fact admits of but one explanation. The complaint commences in a topical affection; I mean a congestion of the abdominal viscera, which is removed by the agency of the secretory process. Nor can it be removed effectually in any other way.

This Broussais and his followers deny. Their remedies are debilitants, lenitives, and revulsives; all of a mild character. Abdominal secretories they reject entirely, or deem them, at least, of little value. They employ them much more as palliatives than as curatives—to obviate unpleasant symptoms than to remedy serious derangement. Their scheme of practice is pleasing from its gentleness; it would seem to be founded on physiological principles; and in theory it looks well. Nor am I willing, like some physicians, to question entirely its whole efficacy. For the cure of fevers of a certain grade, it is no doubt well adapted. But they must be comparatively light. For violent diseases it is too feeble. A powerful foe must be met with ponderous weapons and in vigorous conflict. A missile that will bring down the plover, is but chaff when directed against the eagle or the condor. And such would be the character of the Broussaian practice, if employed against the fevers of the West and South. To attempt to cure those terrible maladies with a dozen of leeches, a few doses of magnesia, cool subacid drinks, and a little mucilage dissolved in water hot or cold, would be solemn trifling. Such at least is my

present conviction. Nor can any thing but the issue of fair and reiterated trials shake it. If I am not totally deceived in all I have seen, and singularly mistaken in all my reflections on the subject, copious secretion from the abdominal vicera is as essential to the cure of the fevers of our great valley, as heat, light, and moisture are to the promotion of the splendid vegetation of our forests. In one respect the therapeutics of the French School are correct and valuable. They are founded on a belief in the locality of fever. But they want vigor to cope with the fevers of the United States. Of physiological medicine generally it is my intention to treat more fully in another publication.

I have remarked that every case of fever, whose origin is clearly indicated by ocular evidence, can be traced with certainty to a local affection. The fevers whose origin is thus palpable are various. They are all such as arise from wounds produced by fire-arms, swords and bayonets, and from other serious mechanical injuries. Of these the topical origin is not questioned. Nor will any one allege that they depend in the slightest degree, for either their beginning or continuance, on a contamination of the blood. Yet, if the chylopoetic viscera be seriously injured, they often resemble very strikingly severe cases of bilious fever. The blood moreover is dark in them, when the pulse is depressed and feeble, and the respiration restricted. To the class of fevers whose origin is obvious, belong also those produced by food and drink, deleterious in their qualities, or taken in excessive quantities, with such as arise from the swallowing of poisons. That these complaints are the result of topical irritation and læsion, in the stomach and bowels, is too plain to be doubted. In confirmation of it, the food is often ejected from the stomach unchanged, and even the drink possessing some of its deleterious properties. Arsenic is found in the stomach, or in the discharges from it, in cases where it has produced death by a malignant fever of short duration; the offspring incontestibly of local irritation. No one, I believe, has ever alleged that that poison, when swallowed, destroys life by entering the blood. Yet the complaint it pro-

duces resembles not a little a high grade of bilious fever. In a particular manner the blood in it is of a dark colour. I have relieved children from fever, convulsions, and wild delirium, by the discharge from the stomach and bowels of the seeds of the Datura Stramonium which they had swallowed, in an unchanged condition. Of this mischief, local irritation was evidently the cause. I have never heard it surmised that, in cases of this description, any portion of the seeds finds its way into the blood. It is only by topical irritation that worms produce fever. They certainly do not enter the blood; yet do they produce a disease which may be pronounced identical with certain modifications of bilious fever. The lividness of some portions of the patient's skin, in this affection, testifies to the dark colour of his blood.

I attended, many years ago, in the city of Philadelphia, a case of disease brought on by swallowing corrosive sublimate dissolved in brandy, which so perfectly resembled one of the worst forms of yellow fever, that the late Dr. Rush, when invited to see it, without having been informed of its cause, mistook it for a case of that complaint. The patient's skin was of a saffron colour, the discharges from his stomach and bowels were black, and he had a hemorrhagy from the gums and nostrils, which nothing could check. His blood, moreover, was exceedingly dark, and did not coagulate. I need scarcely add, that there was no ground to suspect, that a particle of the sublimate had entered the circulation, in as much as the patient began to vomit immediately after swallowing it. The whole was the issue of topical injury of a peculiar character, and in an important organ. Irritate the stomach, in any way, severely and for a sufficient length of time, and fever will be the issue. Persons who are starved to death, or who perish for want of water, die of a malignant fever, the result of irritation and inflammation in the fauces, œsophagus, and stomach. And their blood is dark, and does not coagulate. Yet here is no poison to contaminate it.

Fever often results, especially in delicate persons, from a paroxysm of passion. But the seat of passion is the brain. An

injury done to that organ, therefore is the cause of the complaint. But I need not add, that such an injury is local. The wound produced by surgical operations is often the cause of fever. It also is topical. Hectic fever is likewise the offspring of a local affection; and when a case of fever of any description proves unusually obstinate and protracted, topical irritation and congestion are always the cause. In other words, it is necessarily the product of sub-acute inflammation. But, whatever may be its seat, or peculiar character, inflammation is always local, and cannot be otherwise. In fine, without topical irritation and congestion, as its source, no fever has ever existed; nor, according to the laws which govern the human system, is it possible that it ever can.

But, by the examination of bodies after death, the proof that every case of fever is accompanied by a topical affection, is reduced to demonstration. As already stated, no fever of the kind denominated idiopathic, has ever terminated fatally without such an affection being discoverable in the viscera of the abdomen or the thorax, or in the brain or its appendages. It may be pronounced perhaps impossible for a fever to prove fatal, or to become even highly dangerous, without such a concomitant. As the parts just referred to are the only organs essential to life, some derangement of them would seem indispensable to the production of death. Be this however as it may, such derangement is always found in *post mortem* examinations.

But it is true not only that every fever is the product of some topical læsion. It is equally so that every læsion of sufficient magnitude produces fever. No matter in what tissue or organ the local injury is situated, or by what kind of morbific agent it has been inflicted. Provided it be severe and of some continuance, the system will sympathize with it, and fever be developed. This view of the subject takes from the origin of fever every thing subtle and mysterious, and renders it an object of sense. It does not involve the absurdity of a powerful effect independent of any apparent cause. It therefore simplifies our conception of fever generally, gives to it a common-

sense character, and reduces the treatment of the disease to a definiteness and intelligibility which it does not possess, when conducted under the influence of other principles. The practitioner has before him something palpable, at which he can strike. The foe is dislodged from the ambush that concealed him, and compelled to combat in open ground. His overthrow, therefore, is greatly facilitated.

It is contended that the topical læsions discovered in dead bodies on dissection, do not precede febrile excitement, as its cause, but follow it as its effect; that they are the result of its being thrown on debilitated organs. As this is but a surmise, unsupported by fact, my opponents have no claim on me to reply to it. I am not bound to answer every conjecture that imagination may frame. The task would be equally endless and useless. But as that just stated is a very unfounded, as well as a very common one, and can, I think, be easily refuted, I shall offer a few remarks on it.

From the nature of the symptoms which prevail toward the close of a fever that terminates fatally, it will not be denied that the topical affections alluded to obviously exist a considerable time before death. To the experienced pathologist every manifestation indicates their existence, with a clearness and force of evidence, which he can neither doubt nor resist. Nor is it less palpable that they are the cause of death. These are positions which will not be denied. But wherein consists the difference between the leading symptoms of a case of fever, at its commencement, and toward its termination? I answer, in their degrees of violence, and usually in nothing else. Except as relates to strength and severity, the real characteristic symptoms of a case of fever are the same throughout. Any new symptoms that may appear, are but supernumeraries, not essentials. The principal symptoms continuing unchanged, except as to force, the new ones come in as mere accessories. Were not this the case, the disease would not be the same, in different parts of its course. It would vary from one complaint to another, according to the shifting of its seat and symptoms. Thus would all stability and certainty, as respects diagnosis,

prognosis, and practice be subverted, and confusion would ensue. To illustrate and confirm these views by a few examples.

The symptom which, at the commencement of fever, is but a pain in the head, becomes, toward the end, delirium or stupor. That which, at first, is a pain in the chest, passes afterwards into oppressed respiration, or perhaps effusion or abscess; and that which, at the beginning, is but a want of appetite, nausea, or occasional retching, with a slight degree of uneasiness on pressure, is augmented, as the disease advances, to incessant vomiting, and a fulness of the abdomen, accompanied by intense pain on being touched. To this may succeed, in the further progress of the complaint, and in consequence of the same condition of the parts, only heightened in degree, discharges of a dark gangrenous or secreted matter, or perhaps of blood. Nor is it true that febrile excitement exists before the topical affections which it is erroneously said to produce. As already mentioned, I have never witnessed a case of fever, whose course and history were thoroughly known, in which some of those affections were not in being, before the pulse gave any perceptible indications of disease. That they are deepened and rendered worse by high febrile excitement, is true; but that they are produced by it, is not. This has been long a subject of strict examination with me; and I feel authorized to aver as the result, that, even in the most sudden attacks of fever I have ever witnessed, a skilful and competent inquiry has never failed to draw from the patient, especially if he were intelligent and given to observation, an acknowledgement, that he had experienced some precursory diseased sensations, of a local character.

It is true, that owing to new irritations from undiscovered causes, and consequent determinations of blood to other organs, new topical affections, are sometimes formed in the course of a case of fever. But this is not a common occurrence. The local affections which usher in the complaint, usually remain, to the exclusion of others, until its close. And when new ones do supervene, they are, as already intimated, supernumerary and accidental, and not a necessary part of the complaint.

They do not belong to its radical symptoms. It would be complete without them. But, of what is denominated bilious fever, a topical læsion of the chylopoetic viscera is a more essential concomitant than arterial excitement. The former always and necessarily exists, while, in many cases, the latter does not. In the very worst forms of bilious fever the pulse is not excited; whereas the topical læsion of some of the abdominal viscera is alarmingly severe. Hence, what the late Professor Rush called the "three naturals"—"a natural tongue, a natural skin, and a natural pulse," he considered among the most hopeless symptoms of yellow fever. The great source of error, on this subject, is the belief, that mere arterial excitement and a hot and dry skin constitute fever; while they make, in fact, but a part of it, which, though of deep concern, is, as just stated, by no means essential to its existence. They are as real symptoms, and nothing more, as a headach, a constipation of the bowels, or augmented thirst. They are, in common with sickness at stomach, pains in the limbs, chilliness, diminished secretion, general debility, and other symptoms, the offspring of an original topical affection. In bilious fever and others called idiopathic, there is testimony as conclusive of the existence of such affection in the abdomen, as there is of a topical læsion in the thorax in peripneumony and pleuritis vera, or in the brain in phrenitis. Why do we believe in the existence of a local affection of the lungs in peripneumony? Because the complaint is accompanied by pain and uneasiness in that organ, which are increased by any expansion of it, and the function of respiration is defectively performed. And by an examination after death, the læsion is discovered. And are not bilious and other fevers accompanied by pain and uneasiness in the abdomen, generally much augmented by pressure, with a defect in the functions of the viscera of that cavity? And is not great læsion discovered there, by *post mortem* examination? Both of these interrogatories, as is well known, must be answered in the affirmative. It is indeed true that the topical affection in the abdomen is not, in general, so acutely painful, as that in the thorax. But the reason of this is known. In the latter case

it is a serous, and, in the former a mucous membrane that is affected. And inflammatory complaints in the serous are always most painful. The evidence, then, that there exists no *idiopathic* fever appears to be complete. Such a complaint, as already stated, would be, what has never yet existed, an effect without a cause. The mind, therefore, can have no conception of it, because it has never witnessed any thing like it.

Were I to attempt a definition of fever, I would call it a complaint commencing in a topical affection, becoming, by sympathy, more or less a disease of the whole system, and marked in its course, by several stages related to each other as cause and effect. Were I asked what kind of a topical affection it is? I would reply, that it begins with irritation, or some circumscribed morbid impression, which produces congestion always and often inflammation, in the part irritated. From my present views of pathology, I can no more conceive of fever without previous irritation and local congestion, than of the diffusion of light without a luminous body, or the descent of rain without a cloud. Nor can I conceive of the continuance of fever, after irritation and congestion are removed. There is indeed congestion in every form of disease, whether it be febrile or not. It might be easily shown that the apparent exceptions to this rule are *only* apparent, not real. But to be more particular.

Fever, under the form denominated idiopathic, deranges, in its course, the nerves and brain, before it disturbs the circulatory system. It reaches that system through the medium of the nerves and brain. Its cause, whatever it may be, begins its operation in some part of the alimentary canal; usually I believe, in the stomach. It produces there irritation, which, by a law of our constitution, results in congestion. By the instrumentality of nerves, this topical læsion affects the brain, and disqualifies it for the healthful performance of its functions. In consequence of this, the intellect is injured for the time, and the other parts of the system suffer in turn. They are deprived of their due proportion of cerebral influence, and lose their natural and healthy action. The voluntary muscles fail

in strength, the heart is debilitated, the skin becomes comparatively bloodless and dry, and the secreting tissues, in general, act defectively. The blood driven from the extremities and the whole exterior of the body, in part at least by a spasmodic affection of the tubes containing it, accumulates in the capillaries and larger blood vessels within, and produces the internal fulness and distress that are now experienced. In this round of suffering the lungs do not escape. Deprived of the requisite amount of nervous or cerebral influence, their action becomes imperfect, and they do not sufficiently arterialize the blood. This proves a source of further mischief. It is arterial blood only that suits the purposes of the brain, and of the left side of the heart. Hence the former organ prepares and dispenses to the system still less of its influence, which is perhaps also deteriorated in quality, and the latter is further impaired in its action. Great prostration and distress now exist, and, from an increased failure of voluntary power, more or less of a tremor prevails. Such are the progress of the disease, and the condition of the system, during the first stages of an attack of fever. At length, by a law of the system, which falls within the scope of its recuperative provisions, reaction occurs. If this prove sufficiently vigorous and salutary, the blood is returned to the extremities and the surface, the internal parts are relieved from oppression, the various secretions are restored to a healthy condition, and the fever terminates. The reader will understand that this is intended but as a picture of a single paroxysm of a common intermittent. But it furnishes a correct type of every febrile affection.

To give a full and minute view of fever, this statement should be much more extensive and circumstantial. But, in its present form, it sufficiently serves my present purpose, which is to show that the febrile cause, whatever it may be, does not begin its action on the circulatory system, but reaches it from a local affection in the stomach or some other chylopoetic organ, by means of the nerves and brain. Neither the diminished action of the heart, therefore, nor the consequent accumulation of blood in the large vessels contiguous to it, has any

claim to be considered the first link in the chain of fever. Nor do they constitute, at any time, its entire pathology. They are but secondary and subordinate symptoms of it; inferior parts only of that aggregation of phenomena which forms it. Fever is much more a disease of the capillaries than of the large blood vessels. Its root is in the former, in the character of a topical affection, while nothing but its branches are extended to the latter. Destroy the root, and the branches must wither. But if the branches only be lopped, the root and trunk will remain and vegetate anew. And such would seem to be the defective practice suggested by the theory which represents the seat of fever to be in the large blood vessels.

Am I asked why I believe that a febrile miasm, afloat in the atmosphere, produces its first deleterious effects on the stomach rather than on the skin or the lungs? My reasons are various. The symptoms of fever, as already mentioned, first show themselves in the stomach; throughout the entire course of the disease the stomach is much more disordered than the lungs or skin; on examination after death, it is in the stomach, or some other abdominal viscus, that the fatal læsions chiefly appear; I have seen a malignant fever produced by the direct action of a poison on the stomach, but have never witnessed such a result from any deleterious influence of the kind on the lungs or skin; nor is there good reason to believe that it can be thus induced; the sympathies between the lungs and skin and the other parts of the body are not so well suited to generate a severe febrile complaint, in consequence of a læsion of those parts. A wound in the stomach often produces a disease strictly analogous to bilious fever, while very different forms of disease are the product of wounds of the skin or lungs. Poisonous exhalations that produce disease by acting on the skin effect their purpose by injuring that membrane itself. This is the case with the deleterious exhalations issuing from the rhus radicans, from a West India fruit called the cashu nut, and from other vegetable poisons that might be mentioned. The skin always suffers from their influence first, and the system afterwards by sympathy. But the fever thus produced is

never malignant. To these considerations may be added, that in the treatment of fever, we address our remedies most successfully, not to the lungs or skin, but to the stomach and bowels. To attempt to cure fevers, by acting on the two former parts would destroy the standing of any physician, and would indeed be rank quackery.

I have alleged that the topical affection of the stomach productive of fever, amounts at times to actual inflammation. But I have not yet found full reason to believe, with Broussais and his followers, that this is always the case. A topical congestion short of inflammation will, I doubt not, produce fever. It does not comport with common experience for genuine inflammation to appear and disappear in a few hours. That affection occurs more slowly, and continues more obstinately. It can scarcely be thought to exist, therefore, either in a true ephemeral fever, or a regular tertian intermittent, each paroxysm of which begins and terminates in from six to ten hours, the interval being apparently free from disease. In both complaints the stomach or intestines are affected with local congestion, but not perhaps with real inflammation. Nor is the treatment essential to gastric inflammation requisite in every febrile affection. On the contrary, many of those affections yield most readily to remedies deemed inadmissible in inflammation. My belief then is, that morbid irritation, followed by simple congestion is sufficient for the production of a low grade of fever. But in all violent and obstinate cases, inflammation exists.

On the subject of the existence of intermitting inflammation, and therefore of true inflammation in intermitting fever, I wish to be considered as speaking guardedly. My disposition to deny it is not altogether settled and inflexible. Nor am I prepared fully to concur in it. I hold the matter in doubt, and under further examination.

Every sort of animal action, whether healthy or diseased, is more or less intermitting. We alternately sleep and wake. The muscles, both voluntary and involuntary, act and rest by turns, the glands secrete in the same manner, the process of digestion has its intervals of repose, and there is reason to believe

that the same is true, as respects nutrition. Menstruation and all other natural and healthy evacuations are also marked by periodicity. Nor is the case otherwise with respect to diseases. More or less, they are all inclined to intermit; at least to relax in their intensity. The pain of cholic is strictly intermittent; so is that of neuralgia in every form, and gout and rheumatism are marked by abatements. Nor is this less true of epilepsy and St. Vitus's dance. We talk indeed of *continued* fevers. But the term is used only comparatively. Strictly speaking, no forms of fever are continued. Regular periods of abatement belong to them all. Of hectic, which is an *inflammatory* affection, this is proverbial. Intermitting dysentery, which is also inflammatory, is by no means uncommon. Of peripneumonic and pleuritic forms of disease, the same may be affirmed. Malignant intermittents often appear under these modifications, and require copious blood-letting, calling for its repetition in every paroxysm. Even intermitting apoplexy adds variety to the catalogue of disease. Under the influence of these views, I am induced to pause, and examine and reflect further, before I deny positively the existence of gastric inflammation, in tertian intermittents, merely because the paroxysms are short, and the intermissions complete.

I know we are told, by high authority, that gastric inflammation cannot exist, in these complaints, because they may be cured by stimulating remedies; and because a draught of hot water allays the irritability of the stomach, and checks the vomiting which sometimes attend them.

As respects the point under consideration, these facts are of no avail. Who does not know that hot water applied to them soothes phlegmonic and erysipelatous inflammations? that, in certain states of ophthalmia, water as warm as it can be borne by the eye, without actual pain, forms the safest lotion? and that the best remedies for a fresh burn are spirits of turpentine, alcohol, and volatile spirits of sal ammoniac? Although the practice is hazardous, and ought not to be encouraged, I have seen gonorrhœa cured in twenty-four hours, by the use of a highly stimulating injection. In inflammation of mucous mem-

branes, few remedies are comparable to balsam copaiba. We are told, on what appears good authority, that terebinthinate injections have given relief in dysentery. The best application to an erysipelatous affection is a blistering-plaster. Many other instances to the same effect might be adduced; but these are sufficient for my present purpose. Why then may not the swallowing of hot water and other stimulants be useful in gastric affections, even when the mucous lining of the stomach is inflamed? The dread which the Broussaians entertain of such articles is one of their errors, and weakens their practice. That calomel has often been retained and done much good, in cases where every symptom indicated gastric inflammation, is beyond doubt. So have other purgatives; and even of tartar emetic the same is true. These remedies also are frequently retained by the stomach, while others called milder are rejected. It ought never to be forgotten, that many things which irritate a part in health, soothe it in disease. This may be regarded as a maxim in therapeutics. So radically does disease alter the relations of things. It is thus that the mind in a deep paroxysm of grief, remorse, or despair, welcomes, as a blessing, what would pain and shock it under other circumstances. A wound inflicted with a view to self-slaughter, has been pronounced delightful.

I have already stated, and, to pursue the point further, now repeat (and the sentiment can scarcely be too often repeated) that the practice of Broussais and his school is faulty. In the comparatively light fevers of France, it may be sufficient; but in the gigantic ones of this country, it is too feeble. To attempt to cure the summer and autumnal complaints of the West and South, by a few leeches, magnesia, castor or sweet oil, an infusion of senna, mucilage, and warm and cold water, would be to do nothing; probably worse; instead of affording nature any aid, it might derange her in her recuperative efforts. To cure the fevers of the United States, certainly those of the southern and western sections of it, a much more vigorous practice is necessary. Blood must be drawn, not by leeches, in a dripping process; but by the lancet, in a full stream. Nor will

laxatives produce the requisite effect on the stomach and bowels. The torpor of those organs must be overcome, and their secretory action excited by the most efficient purgatives, and other remedies of great power. Nor must the skin be trusted exclusively to the action of warm and cold water, holding mucilage in solution. Blisters, sinapisms, and other strong rubefacients and revulsives must be often applied to it.

The disciples of Broussais tell us that their mode of treatment prevents the necessity of active puking and purging. In slight attacks of fever, I repeat, perhaps it may. But not in severe ones. As well might they attempt to extinguish a conflagration with a tumbler of water, as to cure our violent and obstinate cases of autumnal disease, by their feeble treatment. Before the patient could feel it, he would be ready for his coffin. In those complaints abdominal congestion is deep and dangerous, and can be removed "tute, cito, et certe," only by copious abdominal secretion. Revulsion cannot be effected at first, but must be reserved for a later period. Nor do I know of any remedies short of the lancet, tartarized antimony, calomel, rhubarb, aloes, scammony, and jalap, skilfully combined, pushed with judgment and vigor, and aided by such minor articles, as the exigency may require, by which secretion can be adequately produced. By the Broussaian practice I know it cannot. I do not mean that all the substances here enumerated should be employed at the same time, or even in the same case. They are the medicines best suited to the general indications; and the practitioner must select, for each particular case, such of them as circumstances induce him to prefer.

It is well known that, in the United States, many of the productions of nature are formed on a larger scale than they are in Europe; and that some of them are larger in the western and southern, than in the eastern and northern sections of the Union. Unfortunately for us here, this is true of our diseases. In the valley of the Mississippi, autumnal fever, no less than the forests and rivers, is of a more gigantic growth, than it is along the Atlantic border. It is more violent and obstinate, and runs its course in a shorter period. The systems of the sick are so

completely absorbed in the struggle with it, that their torpor and insensibility to other impressions are the deeper. Hence the necessity of more prompt and vigorous measures in combatting it. It is not alone the Broussaian practice that is too feeble for it. The same is true of that which is inculcated and pursued in the Atlantic states generally; especially in those north of the Potowmac. The doses of remedies there recommended and administered are too small, and their repetition at intervals too long, to be relied on, in a contest with the formidable diseases of the western states. The fevers and the practice of both regions, have been seen by me. I therefore speak from observation, and know this to be true.

I am not an advocate for doses of medicine inordinately large, in common cases. They are not necessary there. But circumstances cannot always be controlled. Physicians must, like other men, at times submit to them. And, if I mistake not, all experience testifies, that, to produce a given effect, more powerful remedies, or larger doses of them, or both, are requisite in the United States, than in Europe. Of the southern and western sections of our Union, compared to those of the east and north, the same is true. In the former, the practice of medicine, to be adapted to their diseases, and to prove successful, must be prompter, bolder, and more vigorous than in the latter. Some of our eastern brethren, who wield their pens fully as well perhaps as their pills, and who flourish occasionally in the pages of a Periodical, might profit by gravely pondering these things, and endeavoring to glean a little more knowledge from experience and observation, before they proceed any further in harshly and unbecomingly censuring modes of practice, in certain desperate cases of disease, because they are new to them; and with the grounds and principles of which they are obviously unacquainted. It might not be amiss moreover, for those "Collaborators" or Journalists, as the case may be, to recollect, that dogmatically to denounce practice, instead of attempting rationally to correct it by pointing out its errors, partakes as little of good sense as it does of good feeling. The ablest critics are always the most indulgent; and

those who are most abundantly supplied with good things at home, are least inclined to find fault with what is set before them when they are abroad.

In an early part of this article I stated my conviction, that idiopathic fever is a creature of the imagination, and that all fevers are alike symptomatic; that they all begin in a topical affection, and that their subsequent phenomena are but symptoms of this; that this is as true of every fever, as it is that the fever which follows a gunshot wound is a compound of the symptoms of the original injury. Such is the proposition primarily laid down; and I trust that the facts adduced, with the reasonings founded on them, have proved its soundness.

Is any one inclined to ask me, whether I consider all fevers identical? I answer, no. All fevers are, in many respects, analogous to each other; but they are not the same. To maintain, therefore, as some distinguished physicians have done, and still do, that they constitute a unit, is erroneous.

Fevers are varied by sundry causes, some of which are as follows. 1. The kind of tissue in which the original irritation and topical congestion are seated. 2. The region of the body, in which that tissue is situated, and the particular organ of which it makes a part. 3. The nature of the poison or other agent, by which the local irritation is produced. 4. The number of organs, over which the topical læsion extends. Causes which still further modify them are, age, temperament, idiosyncrasy, the condition of the body in relation to general health, and some others. A few remarks in illustration of two or three of the first and principal ones of these, shall close this paper.

1. Fever is now known to be very generally the result of membranous inflammation. That called idiopathic is perhaps always so. Certainly it is always marked by membranous irritation and congestion. There are two membranes, in which the topical affection is usually situated; the serous and the mucous. Fevers situated in the serous membrane are accompanied, as already mentioned, by acute pain in the part. In those situated in the mucous, the pain is less acute, but not less distressing.

In the former the pulse is more hard, tense, *quick* (I do not mean *frequent*) and jerking, than in the latter. The local affection of serous membranes productive of fever amounts always to inflammation. Hence the complaints resulting from it are called "purely inflammatory." A doubt has been expressed whether the local affection of the mucous membrane productive of fever is always inflammatory. The same doubt attaches to the nature of the febrile affection.

To the class of serous fever belong peritonitis, enteritis, one kind of gastritis, one of hepatitis, one of hysteritis and cystitis, pleuritis vera, phrenitis, and a few others. To mucous fever belong the entire family of those called idiopathic, most of which have been already specified; also small pox, measles, scarlatina, one description of gastritis, one of hysteritis and cystitis, influenza, common catarrh, one form of phthisis pulmonalis, and some others of minor importance. To state that these complaints are marked with great variety would be superfluous.

2. Fever, arising from inflammation of the serous membrane, has been stated to be modified by the region where the membrane affected is situated, and the particular organ into whose composition it enters. In confirmation of this, it is only necessary to observe, that peritonitis, gastritis, enteritis and serous hysteritis and cystitis are all different from each other; that inflammation of the serous covering of the liver gives rise to another modification of fever; and that pleuritis and phrenitis are different from them all. The reason of this is found in the different functions and uses of the organ deranged.

Bilious fever again, and all forms of fever, that have their local affection in the mucous tissue of the alimentary canal differ widely from influenza, common catarrh, and the febrile affection which results from inflammation of the internal lining of the uterus and bladder. Dysentery, in which the mucous membrane of the rectum is inflamed, differs from the whole.

3. The nature of the morbific agent, producing the original irritation and topical congestion, modifies not a little the fever that follows. This is illustrated particularly by fevers connect-

ed with the mucous membrane. One kind of miasm irritating the mucous membrane of the alimentary canal, produces intermitting fever; another, yellow fever; a third scarlet fever; a fourth, casual small-pox; and a fifth, typhus fever. And the difference between measles, influenza, and common catarrh arises from the difference in the agents which irritate and inflame the mucous membrane of the nares, trachea, and lungs. The difference between the irritating articles here alluded to, is not a difference of mere strength, but of specific character. No matter, as relates to the present subject, whether the miasm of small pox is stronger or weaker than that of typhus fever; or whether the poison of measles is stronger or weaker than that of influenza. They are each specifically different from the other; and that is all that is requisite to establish the point, for which I am contending. These are matters important in practice, had I leisure to show their influence in that respect.

4. On the modification and complication of fever, arising from the extension of the original affection over several organs, it would be superfluous to dwell. What are called bilious pleurisy, and peripneumonia typhoides are examples of it. In these complaints, the congestion embraces the organs of both the thorax and the abdomen, and renders them different from all others.

In a practical point of view, the doctrine of the original locality of fever is important. Is a physician called to a case of fever, in its forming stage, and is he acquainted with the immediate seat of the læsion that is producing it? By skilfully addressing his remedies to the local irritation, he removes it, and by thus suppressing the fever, saves his patient from great suffering, and perhaps from death. He acts like the surgeon, who, by extracting a splinter, a fragment of glass, or any other foreign substance, before it has produced inflammation, prevents that affection, and every other that might ensue, as its consequence. Nor is it possible for a physician to treat any fever or form of disease rationally, unless he is intimately acquainted with its seat. The surgeon cannot extract the irritating body, if he know not where to find it.

In their mode of operation, curative agents are analagous to deleterious ones. Their primary action is necessarily local. They make their impression on a spot, through irritability or sensibility. By sympathy of parts, that impression is diffused, and rendered more or less general. To this there is no exception; nor can there be, in conformity to existing constitutional laws. It is as true of the most diffusible stimulants, as of the most limited. This position is sustained by the same reasoning, which has been employed in support of that of the original locality of fever. This must indeed be the case, in as much as deleterious and sanative agents are, with but few exceptions, the same, the difference in the result of their action, at different times, arising from difference in the manner, quantity, and other circumstances, under which they are brought into operation on the human system.

Having, in an article on "sedatives," published in a former number of this Journal (vid. vol. II, No. IV, p. 477,) briefly stated my view of the mode of operation of febrile miasms, it would be superfluous to enlarge on that topic, at present. I shall therefore briefly observe, in conclusion, that every febrile agent, of whatever description, must be necessarily an *irritant* to the part on which it immediately acts; and that its *seeming* sedative effects on the heart, muscles, and other parts of the body, are produced secondarily through the medium of the nerves and brain. It injures, by its irritation, the functions of the nerves and brain, and thus deprives the system of its due share of their sustaining influence. Hence the general debility that ensues. A belief in the production of fever, or any other complaint, by a *direct* sedative, is an error arising from a superficial view of the animal economy. It assigns a real effect to an imaginary cause. Appearance is mistaken by it for reality. A more thorough knowledge of that economy, and its true relations to medicinal and deleterious agents, will hereafter consign the hypothesis of direct sedative influence to the same fate with a belief in the elixir of life and the philosopher's stone.

APPENDIX.

In the last number of this Journal, printed long since the foregoing pages were written, an article has appeared, from a high source, on which I deem it requisite to make a few observations. I allude to the paper on the "Theories of Clutterbuck and Broussais," by my able and excellent friend and colleague Professor Cooke. Confident that his object, like my own, is the maintenance of truth, I know him to be friendly to free discussion. I shall therefore offer my remarks, in objection to his, with the less reluctance. In his attempt to expound the mode, in which miasm acts on the human system, in the production of fever, the author says,

"This doctrine (his own) is, that the mode in which this agent, miasmata, affects the system, is by *interfering with the due action of the atmospheric air, with which it is mixed, upon the blood in the lungs;* and by preventing the atmospheric air from producing its usual effect upon the blood, viz. the change of its colour from black to red. To this part of the system this remote cause obviously has the most perfect access, and in this way, as far as my observation goes, every body is acted upon; *every body has the blood more or less darker than usual,* when miasmata are mixed with the air we breathe." (Vid. Trans. Jour. Vol. IV, No. I, p. 114.)

In this extract, the Professor states his opinion on two distinct points; the mode in which marsh miasm acts, in producing bilious fever; and the immediate effect of that action on the blood. It "interferes" he tells us, "with the due action of the atmospheric air" in respiration; and renders the "blood more or less darker than usual."

As respects the latter point my views have been already submitted to the reader; and they are in contradiction of the statement here quoted. From reiterated observation I am convinced that the colour of the blood of persons exposed to a miasmatic atmosphere is not, in the slightest degree, darkened, while their health is retained; nor is the blood of those suffering from fever darkened, except under the circumstances, and for the reasons, heretofore stated. Nor do I rest this assertion exclusively on my own observation. As far as high authority to the same effect may avail, it is not wanting. In speaking of the blood which he drew from his patients, in Philadelphia, during the yellow fever of 1793, Professor Rush says,

"It was, in the greatest number of cases, without any separation into crassamentum and serum, and of a *scarlet colour*." (Vid. Rush's Inquiries, second Edit. vol. III, p. 256.)

Of the bilious "Tertian Fevers" of Minorca, Cleghorn says; "Blood drawn from a vein is most commonly *florid like scarlet*, without any sizey crust; the serum is sometimes tinged with yellow, but oftener *red*, like the *lotura carnium*." (Vid. Cleghorn's Minorca, p. 164.)

The works of Huxham, Sydenham, Riverius, and others contain similar statements. A West-India writer, whose name I have forgotten, contends that yellow fever is the product of a superabundance of oxygen in the atmosphere, because the blood of those attacked by it is *preternaturally red*. I have inquired of many physicians, of ample experience; and they have uniformly assured me, that they have never discovered, during the prevalence of a bilious epidemic, any unusual darkness in the blood of either the sick or the well, except under the circumstances heretofore specified. On these grounds, united to the evidence of what I have myself seen, I reiterate my conviction, that no such darkness exists.

As regards marsh miasm "interfering with the due action of the atmospheric air," in respiration; it can do so only in one of two ways. It must either injure the lungs, by its immediate action on them, and thus disqualify them for the perfect performance of their functions; or it must exist in the atmosphere in such abundance, as to diminish the usual relative proportion of oxygen gas, and prevent the customary amount of that element from entering the respiratory organ, and contributing to the arterialization of the blood.

That it does not act in the latter way, appears from the following facts. Gattoni and Moschati ascertained, by repeated experiments, that the atmospheres of places, where bilious fevers prevail, contain the ordinary amount of oxygen gas. I have performed the same experiments myself, in Philadelphia, and seen them often performed by others, with the same result. In an atmosphere where the amount of oxygen gas is defective, animal respiration is anxious and oppressed, muscular

strength fails, and candles burn with diminished lustre. But lights are as brilliant, with those in health respiration is as free and strength as vigorous, during the prevalence of a bilious epidemic, as at any other time. In fact the only test of the existence of the febrile poison, is its production of fever. It has been my fortune to pass through six epidemic prevalences of yellow fever; and, except when I was suffering from actual disease, my health was perfect, and my capability to endure fatigue greater than usual. If I mistake not Dr. Rush's testimony is to the same effect. In sustaining such a degree of corporeal vigour, high mental excitement no doubt took part.

Marsh miasm, then, does not interfere with the respiratory process, by diminishing in the atmosphere the usual proportion of oxygen gas. Nor is there any reason to believe that it impedes the function of the lungs, by its direct and deleterious action on them. But if it did, the injury would be local, and the fact would so far favour the doctrine I am contending for. Hence the passage quoted offers no solid objection to any view contained in this paper. Again;

In opposition to the belief, that bilious fever arises from an inflammatory affection of the stomach, the Professor expresses himself as follows.

"Bleeding in inflammation of the stomach and bowels is always beneficial. Even when the pulse is weak, it is often practised; and seldom fails to give relief This remedy however is utterly inadmissible in many cases of fever. If inflammation of the stomach were really the first effect of the remote causes, and the cause of all the symptoms observed, it must exist from the commencement; and therefore bleeding ought to be serviceable the moment we perceive the patient to be indisposed; viz in the forming stage of the disease, when the pulse is weak." (Vid. ibid. p. 115.)

As I am confident the Professor does not mean to play the sophist, or argue, in any respect, unfairly, I am induced, by this passage, to believe, that he does not correctly understand the doctrine he opposes. Hence his error. It is not contended, certainly I do not contend, that the local morbid affection, which results in bilious fever, is *inflammatory from its commencement.* On the contrary, I say, that it neither is nor can be so. No morbid affection that can be named is so. Inflammation is necessarily a *secondary* stage of morbid action, and must have a

preceding one, of a different character. It is the immediate product of *reaction;* perhaps I might say it is reaction itself. But reaction, as an effect, presupposes action as its cause. The cause of inflammation is irritation, or some form of deleterious impression. Thus does fever begin. But we do not bleed to remove every such impression. A bullet or a bayonet passes through the body of a man, and produces serious irritation. But we do not draw blood immediately, either to remove this irritation or to prevent its effects. Such practice would be unwise, and therefore injurious, because the powers of the system are already depressed. This, however, does not forbid us to bleed afterwards, when reaction and inflammation have ensued. A person receives a severe burn, and its first effects are, langour, chilliness, and a depressed pulse. To bleed, in this state of things, would be pernicious. But reaction takes place, inflammation is produced, and then venesection is indispensable. Again; an individual falls from a horse, or some other elevated place, and the consequence is, a violent contusion, accompanied by a concussion of the brain. The strength is prostrated, and the circulation weakened. To draw blood now, would be hazardous and improper. But reaction occurs, bringing inflammation along with it; and then it would be hazardous not to draw blood. In these cases the disease does not begin in inflammation; it begins in irritation. But inflammation ensues, and is the cause of the subsequent symptoms, and of all the danger that follows. Hence the complaint is called inflammatory, and remedies to reduce excessive action are steadily administered. It will be observed that, in the forms of disease here alluded to, inflammation always occurs in the places where the original irritation is deepest. To those spots a conflux of blood and cerebral influence takes place, in conformity to a law of the animal economy; and topical congestion and inflammation are the issue. *Ubi irritatio ibi fluxus*, is as strict a truth in pathology, as the axiom, "things equal to one and the same thing are equal to one another," is in mathematics.

These remarks are well suited to throw light on the com-

mencement and progress of bilious fever. The original læsion or deleterious impression, produced on the stomach by marsh miasm, which is the source of all the mischief that follows, is not itself inflammatory, and does not therefore call for blood-letting, with a view to its removal. By its malign influence on the brain, through the nerves, it prostrates the strength and depresses the action of the system, like a gunshot wound, or a severe concussion produced by a fall. But it invites inflammation as a secondary stage of the complaint, for which venesection is an appropriate remedy. It is worthy of remark, that in this, as in other similar cases, inflammation fastens on the seat of primary irritation, produces all the subsequent symptoms, and is the cause of any danger that may arise. Except in congestive cases, in which, by the paralyzing shock of the original impression, the system is sunk below the point of reaction, no fever can terminate fatally without inflammation. If there be any defect in this analogy between the commencement and progress of two cases of fever, one produced by marsh miasm, and the other by a burn, a concussion of the brain, or any other mechanical injury, I am unable to detect it. But if it be sound, then is my position founded in truth, and that of those who oppose me, in error.

In further opposition to the opinion that marsh miasm acts first upon the stomach, in the production of bilious fever, the Professor makes the following statement.

"If this were the mode (deglutition) in which this agent (marsh miasm) finds access to the stomach, the result of its action on the inhabitants of the opposite sides of a stream of water in the neighbourhood of a large marsh, ought to be very different from that which is observed to take place. In such a case, if the wind blow from the marsh across the stream, the miasmata will be carried on to the stream, and there be absorbed. If the water thus imbued were productive of fever, as it must *necessarily* be if the agent applied to the stomach be capable of producing fever, all who use the water of the stream ought to be equally liable to be affected. But this is entirely opposed to the truth of the case. Those on the side of the stream next to the marsh are affected; those on the other escape." (vid. p. 110.)

Using on this occasion the word "necessarily," the author speaks with a degree of confidence, which nothing can justify, but the result of an experiment, fair and satisfactory. But of the existence of such an experiment I have no knowledge. If

he has, it would only have comported with his usual accuracy, to have related it so circumstantially, as to have removed all suspicion of mistake or delusion. In doing this he ought to have made it conclusively appear, that, in becoming mixed with *running* water, (the word "stream" means no less) marsh miasm loses none of its deleterious qualities. Water in motion, beautifully and not inappropriately called "living water," is nature's most efficient purifier. Without it, the earth would be a loathsome pest-house; with it, it is a healthful and comfortable abode. Running water never produces the poison of disease, or any thing else that is foul or offensive. But it destroys many such things. It removes the disagreeable odour from tainted meat, without contracting it itself. There are few, if any odours of the sort, which it will not remove, without being itself infected by them. Nothing of this kind can be more offensive and lasting than the odour of the polecat. Yet running water subdues even that. I once witnessed the experiment. Many years ago a near connexion of mine was so unfortunate as to have some of his clothes sprinkled with the noisome fluid. His first resolution was to burn them. He was dissuaded from this, and induced to bury them in the ground, where they lay several days without being freed from their offensive smell. He next tied them to a stake in a stream of water, which soon purified them.

The condition and effect of stagnant water are very different. Were it not for the motion given to it by lunar influence and winds, the ocean itself would become a mass of corruption. The effect of a long continued calm at sea proves this. Even atmospherical air is rendered deleterious by long stagnancy; whereas motion makes it also an active purifier.

The presumption then is, that running water destroys the virulence of marsm miasm, while stagnant water probably does not; at least not in the same degree. I would drink reluctantly the water of a pond imbued with malaria; but of that of a fountain or stream, under like circumstances, I should have no dread. There is good ground to believe that atmospherical air itself is materially changed, by being absorbed by water.

Of several other gases the same is true. That it is true of marsh miasm also, seems probable from the following consideration. Much of the water, which Professor Cooke considers infected by that poison, is used in culinary processes. In plain terms, it is boiled. While acquiring heat, it emits vapour. Did it contain miasm in its virulent condition, that would likewise rise in the exhalation, and, becoming diffused through the atmosphere, do mischief, precisely as it did before it was absorbed. In this case, those persons using the water, no matter on which side of the stream they might reside, would necessarily suffer. If I am not mistaken, these circumstances show clearly, that the Professor's inference is not warranted by his facts. His objection therefore fails. He proceeds in his objections.

"In the case of the girl (mentioned in a previous part of his paper) affected with fever by whirling round swiftly until vertigo and nausea were produced, it is manifest that the action was not immediately upon the stomach.

"In the case of the girl (also mentioned previously) who fell into a quartan ague which continued a whole winter, on being terrified by the unexpected sight of a dormouse, and which was produced a second time by the same cause, after it had ceased, it is perfectly obvious that the cause could not have acted directly on the stomach.

"In cases of fever produced by a blow on any part of the body, which often occurs, the cause has no more immediate relation to the stomach, and cannot possibly act upon it directly." (Vid. p. 111)

As embracing matters of fact, these statements are true; but they do not avail the professor in his argument, because they have no immediate bearing on his subject. He does not view them in their true character. The ground of his mistake moreover is plain. He does not make the proper discrimination between the several kinds of causes, which cooperate in the production of fever. The causes which he here specifies, were only the *exciting* causes, bringing into actual existence fevers, to which the subjects of them were *predisposed*, in consequence of the previous action of their *remote* causes. No one will allege that vertigo from whirling round, a fright from a dormouse, or a blow on any remote part of the body acts *directly* on the stomach. But from the shock they give to the brain, the two first directly, and the third through the nerves, and the derangement which they there produce, they

act very powerfully by sympathy, on any important organ, which is already debilitated and predisposed to disease. Is the stomach thus predisposed, in consequence of the influence of marsh miasm, as appears to have been the fact, in the cases of the two girls? The affection, of which that poison is the remote cause, is produced. The patient suffers from a quartan, or some other form of bilious fever. Without such predisposition that complaint cannot be excited, although some other, to which the system inclines, may. The enlightened pathologist need not be informed, that a mere exciting cause has no peculiar fitness to produce one sort of disease more than another. It only develops that malady to which the individual is inclined: and in every person there is, at all times, a leaning to some one form of complaint rather than to any other. Is that form the gout, and is the exciting cause applied? gout follows. Is it scarlatina? scarlatina is developed. Is it pulmonary consumption? the same is true of that affection.

The following case fell under my notice in the city of Philadelphia. Yellow fever was prevailing epidemically, and, of course, every one in the place was in some degree predisposed to it. A quarrel occurred between two individuals, one of whom gave to the other a severe beating. The consequence was, that, in a few hours afterwards, the latter was attacked with yellow fever, of which he died. His body was opened, and his stomach and small intestines were in a state of high inflammation. Yet, in the battle which had excited the disease, not a blow had been placed on any part of the abdominal region. They had been confined entirely to the head and neck. Exposure to fatigue, rain, and the atmosphere of the night, is a common exciting cause of yellow fever, in which the abdominal viscera are always effected, and generally inflamed. Yet such exposure does not *primarily* act on the stomach or intestines. No exciting cause thus acts, except improprieties in food and drink. Notwithstanding this, they all contribute alike to the production of inflammation, or some other serious læsion, in the viscera of the abdomen. These considerations compel me to believe, that the Professor is unfortunate in his objections to the gastric orgin of bilious fever.

Art. IV.—*Observations on the Fever which prevailed at Alexandria, Louisiana, in the Autumn of* 1830. By Josiah Hale, M. D.

Before entering on the immediate subject of this paper, it may not be inappropriate to premise a few observations on the topography and climate of this place.

Alexandria is situated on the south-west side of Red River, in latitude 31° 15′, one fourth of a mile below the falls or rapids. The river is here about two hundred yards wide, and of sufficient depth to admit of steamboat navigation, at all seasons. The rapids are formed by a ledge of rocks, of recent formation, and soft friable texture, which traverses the river obliquely from East to West. The entire descent in a distance of half a mile, is seven or eight feet, the principal part of which is made up by the last or lower pitch, which is sufficiently abrupt to break the water, at a low stage of the river, so as to be heard at a considerable distance. Bayou Rapide enters the river from the west at the foot of the falls. The banks of Red River, except at the pine bluffs are soft and yielding, wearing away in some places, and forming in others; in this way a sand-bar, extending from the falls to a point opposite the centre of the town, and fifty yards in breadth, has been formed on the North-east side of the river: this bar is always naked at a low stage of the water.

The south-western bank of Red River, on which the town of Alexandria is situated, is about 30 feet high, above low water, and is entirely composed of alluvion; the same formation extends two miles south, to the pine lands of Bayou Bœuf. The surface of the ground descends at the rate of about one foot in one hundred, as far as the back limits of the town and the margin of a small sluggish bayou or drain, half dry during summer; from this point the surface rises, but more gradually, until, at the distance of half a mile, near some small bayous, it is as high as at the river bank. The north-eastern bank of the river is a high pine bluff, on which is situated the village of Pineville, containing about eighty inhabitants.

The town of Alexandria extends half a mile along the river and not more than one fourth this distance back: the streets are spacious and laid out parallel and at right angles with the river. Front, or Water-street, and the public square are the only parts closely built up. The houses are generally framed and have one or more galleries, and are painted white: the back yards and lots are generally fenced with plank; in the former stands the cistern, a large wooden vessel eight to twelve feet in diameter and as many in height; this is filled with rain water, caught from the roofs of the houses. Cistern water soon looses the peculiar vapid taste of rain water and becomes as lively and pungent as the finest spring water; this is the resource for drinking water, as well as for all domestic purposes: the river water being strongly impregnated with saline substances, and for a great part of the year, holding in solution a quantity of red mud, is entirely unfit for domestic use.

In May and June, 1828, Red River overflowed its banks, from twenty miles above this place to its mouth, except at the pine bluffs, which are high; no such event had occurred since 1802, when the river rose to a still greater height. At this place the water was from one to two feet in depth on the bank of the river; currents and counter currents, directed by the buildings and fences, deposited large beds of sand in the streets and open lots; excavations were also formed in a few places, but after these were filled up and the streets leveled, the latter were found to have gained an accession to their surface of from six to twelve inches, while the enclosed lots and back yards remaining at the former level, became comparatively low, and consequently the recipients of much of the rain water which falls in the streets. The dampness of these situations is further augmented by the constant leaking of the cisterns. Musquetoes are more or less troublesome, during the cool part of the day, from March to November; the inhabitants guard against them by means of bars of netting. Prior to the inundation of 1828, the streets were shaded by the Pride of China trees, (melia azedarach,) which grew abundantly on the side walks; these were all destroyed by the deposition of sand about their roots at this inundation.

The population of Alexandria, in winter, is about five hundred; in summer, this number is reduced one half by emigration to the pine woods. About the first of June, more than half the families retire to their summer seats in the pine land, for the advantages of health, which they enjoy in a high degree; here they remain until about the first of November. Those who continue in town, consult their safety and comfort, by avoiding the sunshine and keeping cool, as far as practicable; they are generally regular and temperate in their habits.

At the time of the inundation in '28 apprehensions were entertained of violent summer and autumnal diseases; these fears and the inundation happening at the usual time of migrating to the pine woods, caused an unusual number of inhabitants to leave the place; but these apprehensions were proved by the event to have been groundless. Occasional light showers and fresh breezes prevailed throughout the summer, and the season was healthy. The summer and autumn of 1829 were wet and cool, and no less remarkable for the prevalence of general health. This remark does not apply to a portion of the settlement on Bayou Rapide and the upper part of Bayou Robert, as considerable sickness occurred there, with unusual mortality.

The commencement of the last summer was healthy. May, which, as well as September, may be regarded as a summer month in this climate, was remarkably warm and dry; June was also warm and dry: before the middle of this month the mercury, in Fahrenheit's thermometer was observed to stand at 95° in the shade, and continued for several days in succession to rise above that degree. In July there fell some light showers in this place and other parts of the parish, but they were of limited extent, and were not followed by any considerable reduction of temperature. The crop of cotton was uncommonly fine; that of the sugar-cane on several plantations, suffered from the drought.

The winds, in this locality, are variable throughout the year; that from the north-west is the most steady as well as the strongest; it blows for a greater number of days in succession, than any other; winter and the early part of spring are the seasons

when it prevails. During summer, refreshing breezes spring up in the south-east between 8 and 10 o'clock in the forenoon, and continue to blow from a few degrees behind the sun until four or five in the afternoon, when they die away in the south-west. These breezes greatly mitigated the heats of the last summer. I recollect feeling quite comfortable one afternoon, while sitting in the back door of my office, while the thermometer a few feet above my head, indicated a temperature of 99° —the house is protected by a gallery ten feet in width on each side. Easterly winds generally blow in the latter part of summer and autumn; they are the most disagreeable of the climate, being cool, damp and chilling, and are frequently attended by dense fogs which hide the sun until eight or nine o'clock in the morning. The nights are cool and refreshing, and the dews heavy. Rain very seldom falls during the night, from the first of June to the first of September: the heaviest showers generally fall some weeks after the equinoxes.

During the past autumn, easterly winds prevailed to an unusual degree, and the fogs which attended them were rendered doubly dense by the admixture of smoke wafted from the pine woods, which from the extreme drought continued to burn throughout the autumn: this smoke, although strongly impregnated with the odour of burning pitch pine, proved no antedote to the poison of our atmosphere.

The last spring and early part of summer, as is usual in a dry year, were healthy: in August and September a number of cases of bilious fever occurred in the vicinity, several of which terminated fatally—one within thirty hours from the attack; it was produced by a powerful exciting cause. It has been long observed that a hot and wet summer and autumn are distinguished by the prevalence of bilious fevers of violent forms, and those of an opposite charater by the prevalence of general health.

As regards the source in which our late epidemic had its origin, I believe but little doubt can exist, for, with the exception of the high degree and long duration of the solar heat of the past season, no apparent cause for its production, was ob-

served, which had not obtained to as great or even a greater degree, in former years, not distinguished for unhealthiness. In 1828, the fresh sand in the streets did not become perfectly dry before the commencement of autumn, and subsequently moist surfaces of this sand were daily exposed to the sun's rays by scraping and levelling the streets.

The inhabitants of Pineville have generally suffered from disease at the same time and in an equal degree with those of Alexandria; during the last season they enjoyed an entire exemption from disease. The adjacent county was also remarkably exempt from disease during the prevalence of our late epidemic. From these considerations it appears that some cause existing and operating within the limits of the town, must have obtained; a cause from which the town itself has been exempt in former years, and which did not operate beyond its limits during the past season.

Having said thus much on the topography and climate of this place, and having given some account of the state of general health, for several years past, I shall now proceed to make some observations on the causes in which the late epidemic had its origin.

The remote cause of intermitting and remitting fevers is known to be a product of heat, moisture and dead organized matter, existing together in certain unknown quantities and proportions. The name by which this agent is designated, is miasm or miasmata; of its precise nature nothing is known, the present state of chemical science affording no means of examining it. We only know that it exists, by its deleterious effects on the animal economy. As emanations from neither of the two ponderable prerequisites in the production of miasm, in any state of decomposition or analysis with which we are acquainted, possess any of its deleterious properties, as manifested on living animals, it is probable that it is by re-combination of their elements, aided by heat, under favorable circumstances, that this poisonous agent is produced.

The remote cause of malignant fever seems to be produced in circumstances pretty similar to the above: so much so, that

it has even been supposed to be the same agent in a greater degree of concentration or force; but if these circumstances be minutely examined they will be found materially different. For the production of the miasm of malignant fever, a higher degree and longer duration of solar heat is necessary than that of intermittent fever, and at the same time a less quantity of moisture is required. This is so strikingly the case that some have supposed that even none at all was necessary. Here then, we find the elements in different proportions, and may reasonably expect products of different natures: such is the result in chemical processes, the same elements, by combination, producing different substances, according to their different proportions, or the different degrees of force in the generative process.

For the production, therefore, of the miasm of malignant fever a more suitable combination of circumstances can scarcely be imagined than was afforded in Alexandria during the last summer and autumn. The surface of the earth, composed in a great part of vegetable debris, too level to admit of being fully drained; the back yards, and beneath the ground floors of the houses, lower than the adjacent streets, saturated by the spring rains and kept damp by the constant leaking of the cisterns; add to them the filth and moisture common to like situations in all towns, and but few prerequisites will be wanting to produce malignant fever in any portion of the globe. The materials of the houses and fences, well calculated for the reflection of solar heat, unprotected by a shade of any kind, acted on by the direct rays of an almost tropical sun, reflected in a thousand directions, produced a degree of heat, which added to the above circumstances, are sufficient to account for the appearance of our late epidemic, and should excite apprehensions of a like visitation whenever a similar concurrence of circumstances shall again take place.

I will now proceed to give a sketch of the rise, progress, and termination of the epidemic.

On the 17th of September the steamboat Saratoga arrived here, after a passage of three days from New Orleans, (where the yellow fever was then rife,) with the first mate sick on board.

who was reported by the physician who attended him, to have the yellow fever. I saw this patient on the night of the 20th, a few hours before he died: he had been bled twice, after he arrived; on the third and fourth days of the disease, his respiration was attended by a hoarse, deep, sullen sound, which was heard in the streets by persons passing; he was able to rise in bed, but not to comprehend the most simple inquiry; he was vomiting black matter, which was thrown up by considerable effort; the skin was marked with petechiæ. Two days after the arrival of the Saratoga, the second mate and the carpenter were attacked with chill and ague, soon followed by fever, and this, in a few hours, by copious perspiration, pain in the head and back, with watery, red eyes, &c. Although I saw the mate about eight hours after the attack, such was the reduction of force in the pulse that I did not think bleeding admissible.—These two cases were treated with calomel, &c. The gums were swollen at the end of the first twenty-four hours, and in a week the patients were convalescent. One of the hands of the same boat sickened and died in town, with black vomit; he had no medical attendance. These were the only cases which had any connexion with the Saratoga, nor was there any occasion to suppose the disease was in any instance communicated from one person to another. The epidemic now continued to prevail with greater or less violence, according as the weather was hot and dry, or moist and cool, until December, when the cold weather put a stop to its further progress. All persons, of whatever country, age, sex or color, were more or less liable to be attacked, except young children. I saw no child under four years of age with the fever, nor did any particular temperament or habit appear to be more obnoxious than another: it attacked equally the most robust and plethoric,* and the most weak and debilitated. The whole number of cases was about fifty, of which fifteen terminated fatally, two of which had no medical attendance. I saw four-fifths of them in some stage of their progress; Dr. Cuney also saw a large proportion of

*The robust and plethoric, appeared to be the most obnoxious to the disease. Note by R. R. Cuney, M. D.

them. The mortality, it must be observed, was great; a variety of circumstances, independent of the violence of the disease, contributed to this result, among which may be mentioned the heterogeneous character of the population, at this season of the year; the belief entertained by many of the contagious nature of the disease, and consequent difficulty of procuring nurses and attendants for the sick, and above all, the resemblance of the disease, in the early stages, to the ordinary fevers of the season, which caused many to postpone calling medical aid until the disease had become so unmanageble, as to bid defiance to every attempt to subdue it.

Having premised thus much, I will now proceed to give a more particular account of the symptoms and mode of treatment adopted, and also describe the post mortem appearances in several subjects examined.

SYMPTOMS.

For the sake of perspicuity I will arrange the symptoms under three heads, or divide them into three classes, corresponding with the different stages of the disease in which they were prevalent. They were, first, the cold stage, or that of internal congestion; second, the febrile stage, or that of open excitement; and third, that of debility, or decline of the vital and natural forces.

1. The congestive stage resembled the corresponding one of the numerous class of pyrexiæ of Cullen; it generally commenced with a feeling of langour or lassitude, which sometimes continued one or two days, but more frequently had a much shorter duration; at other times the more violent symptoms manifested themselves suddenly; the patient, in some instances, would go to bed in apparent health, and be awakened between 2 and 6 o'clock in the morning with sickness, pains, &c.; children were frequently awakened by vomiting, after having displayed unusual vivacity the preceding evening. A sense of coldness, rarely amounting to a distinct chill, was frequently the first indication of disease; this was often so slight as not to be perceived, except on exposure to fresh air. In a few cases rigors ushered in the disease; pain in the head, back

and breast were often simultaneous with the chilliness, and continued throughout this and the succeeding stage, and in some instances into the next. The pain in the head was sometimes seated over one or both eyes, at others it passed from one temple to the other; by one patient it was compared to the boring of an auger through that part of his head. The occiput and nape of the neck were sometimes the seat of pain. The pain of the chest was seated immediately under, and in the direction of the sternum; this was always obstinate and difficult to remove: the eyes were dull, heavy and watery; the face pale and of a dirty appearance. After these symptoms had continued for some time, or in some instances from their first appearance, sensations of warmth would begin to alternate with those of cold; warm flushings, at first transient and partial, would, by degrees, become more permanent and general, until they predominated over all feelings of cold. In some cases, particularly late in the epidemic, after all sense of cold had yielded to that of heat, the chilliness would again return: this was seldom at such an interval, as to give the disease the appearance of an intermittent.

2. The second stage, with the subsidence of the chilliness and the prevalence of general heat over the surface, was usually marked by a flushed face, and a strong, full and frequent pulse; the eyes were red and watery; the tongue differed but little from the healthy state; a light coat of a pale yellow or cream color covered the middle and back part. The pains which attended the first stage were aggravated in this. After this state had continued for some time, rarely more than a few hours, a profuse perspiration would take place, and the patient be literally bathed in the fluids of his own body. In some cases this stage was but feebly marked, the chilliness would subside, the pulse and the temperature would be slightly raised, the surface become partially bedewed with perspiration, and the patient pass into the third stage of the disease.

3. The commencement of the third stage was marked by the subsidence of many of the preceding symptoms and the rise of others; sometimes with a deceiving tranquillity while the

patient was awake, but his sleep was perturbed and unrefreshing: a constant disposition to sleep attended some cases; an air of resignation, or rather content, characterized the whole demeanour: the cheeks retained much of the florid complexion which attended the former stage: the skin was smooth, polished and shining; that surrounding the eyes, for some distance, was of a chlorotic or leaden hue, which, with the watery red, or in some cases fiery red eye, imparted a peculiarly wild expression to the countenance. This dark color frequently appeared with the first symptoms of the disease, and disappeared with the last. In two cases the skin and white of the eyes assumed the yellow appearance common in the bilious fevers of the country; only one of them was examined after death, the appearances of which were similar to those described below, except that the adipose substance was yellow. The pulse was moderate, differing but little, in many cases, from that of health; it was sometimes faster, but often slower than natural; considerable irregularity was sometimes observable. Respiration was but little hurried or labored, but was often irregular, and interrupted by frequent deep sighing, of which the patient appeared to have no consciousness. The coat on the tongue extended, and became thick, and of a deep yellow, was smooth and moist, and on being scraped off with the nail, left the surface of the tongue rough and shaggy, but of the same color; the part scraped, a few minutes after, could not be distinguished from the rest. In the more violent cases, the tongue assumed a dark brown color—in some it partook of a shade of green—it sometimes appeared soft and swollen, and bore the impressions of the teeth both above and below. The voice was hoarse and husky. Sordes, in small quantities collected on the teeth, in a few cases, a short time before death. Thirst universally attended throughout every stage of the disease: cold water was preferred by the patient to all other drinks. Nausea and vomiting were universal attendants; vomiting was frequently among the first symptoms, and was seldom later in making its appearance than the developement of fever; it was sometimes violent and incessant from the beginning, but more frequently moderate and

at considerable intervals; it became more violent and occurred at shorter intervals, as the disease advanced, until the patient was harrassed by efforts renewed every few minutes. Vomiting was generally attended with straining, but in some cases, a short time before the fatal termination, it was effected with but little effort, and was then attended with but slight nausea. The matter thrown up consisted at first of a little viscid, transparent phlegm, mixed with the fluids drank; this phlegm, as the disease progressed, was found floating in a fluid of reddish brown color, which gradually became deeper until it assumed the appearance of weak coffee: the phlegm now gradually disappeared and was succeeded by brown granular particles; the whole, when thrown on the sheets, gave precisely the appearance of the stain of coffee grounds. In one case a blackish fluid was thrown up in much larger quantity than the ordinary black vomit, and contained the same dark particles. After death, the gall-bladder was found distended with a fluid similar to that thrown up, but destitute of the dark particles—these were found abundantly in the stomach, mixed with the black liquid. The bowels were generally torpid from the commencement of the disease, requiring large doses of medicine to move them. Late in the season, when bleeding was more frequently and effectually employed, this torpor was seldom observed, and the milder cathartics would readily purge. Worms of the lumbricoid sort, were general attendants in young subjects—they were discharged by the mouth in several cases. A diminished secretion of and scalding sensation in voiding urine were frequently observed, and in several cases an entire suppression lasted for several days. Deafness generally attended. The occurrence of hæmorrhage was one of the most striking peculiarities of the epidemic; it never appeared before the third stage of the disease, and frequently not until late in that, but seldom failed to take place before the termination. Bleeding from the nose, fauces, gums, blisters, and every part where the cuticle happened to be broken, was always troublesome, frequently profuse, and in one case appeared to hasten the fatal event. The blood was of a light venous color, in some instan-

ces, approaching the arterial. The cerebral functions were but little disturbed; slight delirium sometimes attended the first access and highest degree of fever; this was generally transient, but in some instances might be detected until the termination of the disease. As the fatal event approached, the functions of the brain, like those of the other important organs, became more deranged, and finally ceased to be performed. No distinct crisis was observable. From the 5th to the 9th day the bad symptoms began to abate, and in two or three days the patient was convalescent; or an aggravation of the unfavorable symptoms took place, and the patient died, generally, from the 5th to the 9th day; one case was protracted to the 14th day. The prognosis, early in the disease, was uncertain; no one symptom could be regarded as decisive as to the event; it was only by grouping a number of circumstances together that any judgment on the subject could be formed. Frequent and long continued vomiting, without any thing being thrown up, except the drinks and a little viscid phlegm; great soreness of the epigastric region, and deep pain on pressure; the coat on the tongue being dark and extensive; a craving for an unusual article of diet, were unfavorable; as were irregular respiration, frequent sighing, with great torpor and disposition to disturbed sleep; a suppression of urine, or a great diminution of the quanity was unfavorable: in one fatal case the urine was bloody and discharged in larger quantity. A favorable prognosis was formed from the mitigation, or subsidence of the bad symptoms. Convalescence was slow, but regular; only one relapse took place under my observation.

TREATMENT.

Of the various remedies employed in the late epidemic, the first in importance and efficacy was *blood-letting;* when employed in time this remedy seemed to exert a most salutary influance. Called early, I generally opened a vein, as soon as excitement was fully restored, or when this did not promise to be complete, before, even when considerable coldness of the extremities remained. In such cases, after drawing a small quantity of blood, if the pulse did not rise, or if the chilliness in-

creased, I bound up the arm and waited the rising of the pulse: this generally took place sooner or later, and the bleeding was repeated with more freedom; a third bleeding in such circumstances was sometimes necessary; but in cases where the excitement was well developed, the pulse full and strong, I bled from a large orifice until a distinct impression was made on the pulse, and in some instances to fainting. In a few cases, early bleeding appeared to cut short the disease and so completely relieve the patient, that after a few days he could scarcely believe he had been attacked with any thing more than an ordinary fever.

So rapidly did the state of excitement pass off, in many cases, that bleeding could not be employed after the physician was called; a few hours was often sufficient to reduce the patient to a condition that afforded no hope of benefit from the remedy. Bleeding appeared to be injurious, in one case, twelve hours after the attack, when the pulse continued full; but when applied in time, before the system began to fail under the disease, it was followed by the happiest effect: the pain in the head and limbs would abate, the respiration become regular, the skin moistened with perspiration, and the patient fall into a quiet sleep. I lost no patient whom I saw sufficiently early to bleed with confidence, but several treated by other physicians, in which the remedy was promptly and judiciouly employed, died at about the usual period.

Cathartics.—A variety of articles, of this class of medicines, were used; among which the most generally preferred was calomel; its well known effects on the secretion of the liver, rendered it highly eligible in a disease, where the function of that viscus appeared to be entirely suspended. Calomel was given in doses of from a scruple to half a drachm, and in some cases a still larger quantity, repeated every four, six or twelve hours, according to the urgency of the symptoms. In these doses it generally purged moderately, but in some instances it was necessary to assist its operation at first by castor oil, senna, manna, &c., it never produced hypercatharsis. The evacuations from the operation of calomel were generally thick, con-

sistent and dark, such as in the bilious fevers of the country we are accustomed to regard as highly favorable, but in our late epidemic no favorable prognostic could be formed from them. In some cases the evacuations were fluid, watery and even limpid; endeavors were made to arrest these by combining opium with the calomel, by substituting other cathartics where they could be retained on the stomach, as aloes, rhubarb, scammony and the comp. ext. of colocynth, in various combinations, but these were all found to be ineffectual. As the thick, dark evacuations were never indicative of a favorable issue, so these thin, watery stools were not of an opposite one; a number of recoveries took place, when the evacuations were throughout of this last character. The effects of cathartics on the disease were never very striking, so completely were the sympathies between the first passages and the rest of the system, broken up and destroyed, that purgative medicines seemed to be followed by a mere local effect. One effect of calomel, deprecated by some practitioners, and hailed as a favorable omen by others, salivation, was not difficult to produce; it frequently took place within twenty-four hours after the exhibition of the first dose of the medicine, this was equally the case whether the patient had been reduced by the disease, or had been depleted by the lancet. In a few cases even the largest quantity of calomel above mentioned, would not salivate; in that of the lamented F—, a slight sponginess of the gums and a barely perceptible mercurial odour in the breath, for a day or two, was all that could be effected. Salivation, like catharsis appeared to exert no influence over the disease, it was rarely excessive and never violent, until after the patient became convalescent. Late in the epidemic calomel was less relied on. When called early I generally gave sulph. mag. 3ss. with carb. mag. 3ij.; this repeated after a short interval, rarely failed to operate freely, and frequently produced dark consistent stools; calomel was afterwards given, either alone or in combination with some of the above-mentioned cathartics.

Vesicatories.—The great irritability of the stomach seems to indicate the application of a blister over the seat of this vis-

ous; they were early applied to the epigastric and right hypochondric regions, and although in some cases they appeared to mitigate the nausea and vomiting, they never entirely relieved them, and in many cases they appeared to do no good. So soon as the arterial action was reduced, if the pain in the head continued, a blister was applied to the nape of the neck; this rarely failed to relieve the pain at once. In one case attended with violent pain in the back, accompanied by uterine hæmorrhage, not an uncommon attendant, a blister to the lumbar region was of service, but in general warm stimulating frictions afforded relief to the pain of this part. When reaction was partial, the extremities remaining cool after the rest of the body had become preternaturally warm, blisters to the legs and arms were of service in equallizing the excitement. When the vital powers were failing and the skin covered with a clammy perspiration, sinapisms were applied to the extremities. Frictions with hot brandy, &c., were also had recourse to in these circumstances. Blistered surfaces showed an unusual tendency to heal, in some cases; after a day or two the discharges would cease, and the only vestige of the blister would be a dark colour of the skin; if the plaister was then reapplied, it drew well and the blister discharged as usual; in other cases when the plaister was not reapplied, the blistered surface, after remaining dry for a day or two, would suddenly become moist and continue to discharge as in ordinary cases; in this state it was liable to bleed. Strangury was a common effect of blistering, it was often distressing but soon yielded to the ordinary remedies. Sponging the body, when the temperature of the surface was above that of health, was practised and was grateful to the patient; for this purpose cold water, vinegar and water, and a weak solution of the mineral acids were used.

The sulphate of quinine was given in the third stage of the disease, in several of the most violent cases, but here, like every other medicine, it appeared to do no good; it was either immediately ejected from the stomach, or retained a short time and then thown up, without having produced any bene-

fit; in other cases, however, of a more tolerable character, given in doses of from four to eight grains, every two to four hours, it appeared to be of service: it increased the volume of the pulse, raised the temperature of the surface, and imparted tone to the stomach; it was sometimes given in combination with calomel.

Other remedies were occasionally employed, among which were the spirit of turpentine, charcoal, and the ordinary ones to relieve vomiting, but as these were followed by no decisive effects, they need not be detailed here.

SECTIO CADAVERIS.

Post mortem examinations were made, from six to twelve hours after death. The body appeared emaciated and stiff; the back and other depending parts were of a purple hue, from the subsiding of the blood. In one subject the blood continued to flow from a blister on the back of the neck, until, from appearances, several pounds must have been lost. Hæmorrhage from the gums and fauces had been profuse in this case, for forty-eight hours before death, and probably hastened the event; the body was nearly emptied of its blood. Black vomit frequently flowed from the mouth and nose after death. From the apparent exemption of the brain from disease, as manifested by the state of the mind, and the little time allowed for dissection, that viscus was not examined. The thorax exhibited but little remarkable, the right side of the heart and venous system were generally engorged with blood; the diaphragh was elevated or pushed up into the cavity of the thorax. The abdominal viscera showed the greatest ravages of disease, and of these the stomach, as it appeared to bear the chief burden of the disease while life continued, so it exhibited the principal vestiges of its effects after death. The stomach was generally found of its natural size, but in one subject it was contracted to a smaller diameter than the duodenum; the mucous coat was thrown into elevated rugæ, throughout its whole extent, and exhibited inflamed patches, often of several inches in diameter; these patches were of irregular form, several of them appeared frequently joined together, forming one of large size;

they were generally most numerous near the pyloric orifice, but in some instances all parts appeared equally occupied by them; the rugæ were inflammed throughout, giving the parts not covered by the inflamed patches, when the stomach was spread out, the appearance of being marked with red lines; these inflamed portions were of a deep arterial colour. The mucous coat appeared thickened and soft, and was easily separated. In the case of Mr. G. B. all the coats of the stomach were softened, and appeared rotten; Dr. Cuney, in raising the stomach for examination, tore it without using any extraordinary force. The duodenum, in its peritoneal coat, exhibited a redder and more vascular appearance than natural, the mucous coat was also red and inflamed. In one instance a portion two or three inches in length and half the diameter of the intestine was of a dark brown color; this appearance pervaded all the coats and was probably the commencement of sphacelus. The other small intestines exhibited a few patches of inflammation. The liver was engorged with blood; in one instance the anterior, and in another the posterior surface exhibited an inflamed patch as large as the palm of the hand. The gall bladder was distended with a thick and viscid bile, so thick as to run very slowly from the fingers when raised upon them, and could not be forced through the bile duct; its color appeared to be black when in a mass, but when spread on linen was a dark greenish brown. The parietes of the gall-bladder were thickened and the inner coat appeared inflamed. The kidneys, in the only case where they were examined, that mentioned above in which bloody urine was discharged, were enlarged, soft and of a uniform red color throughout.

Alexandria, La. December 24th, 1830.

ART. V.—*Observations on Dropsy.* By HUGH HUGER TOLAND, M. D. of South Carolina.

MAN in a rude and unpolished state, deprived of all the luxuries of life, and scarcely able to obtain a comfortable existence, whose intellectual faculties are uncultivated, and exert comparatively little influence over the physical energies of the system, is, as far as I have been able to ascertain, almost entirely exempt from dropsical affections. But when we take a view of him in a state of civilization the scene is reversed. Being tenderly raised and subject to the wrecking influence of inordinate repletion, stimulating potations, inactivity of body, mental excitement, and other causes of a local and accidental nature, he is rendered obnoxious to this and many other diseases of a complicated character, for the removal or palliation of which many remedies are required, and great skill in their administration called for.

Although the subject of this paper has received much attention, and physicians of the greatest respectability have been engaged in its investigation; and although much benefit has resulted to the unfortunate victims of this disease, by their unwearied exertions, something more yet remains to be done in order to enable them to treat successfully all its aggravated forms. In some of these publications, the treatment of this disease is detailed in a manner calculated to inspire great confidence; but when resorted to in a complicated case, it fails, and but too often under the joint influence of all the remedies recommended, it progresses regularly to a fatal termination.

Some writers suppose dropsy to be hereditary; and it certainly as well deserves this appellation as most other diseases: persons of a phlegmatic temperament, being most liable when placed under the circumstances producing it. As exciting causes, have been enumerated every circumstance calculated in any way to produce a feebleness of constitution. But I am disposed to doubt the correctness of this opinion; having never

seen a case of ascites, or general anasarca, in which considerable chylopoietic derangement did not exist: one or more of the vital organs having refused obedience to the laws by which they are governed in a healthy state. Moreover the well known effects of intemperance, and neglected or mal-treated febrile diseases—the most fruitful causes of dropsy—should in my humble opinion, be sufficient to convince every one at all conversant with the subject, that a case of idiopathic dropsy rarely occurs.

Almost every case which has come under my notice, could be proved satisfactorily to have depended on one or all of the causes above mentioned; all of which produce an unsoundness of the digestive organs, by which the fluids are vitiated, the system badly nourished, and the circulation disturbed.

In the section of country where I lately resided, intermittent fever prevailed extensively, during the summer and autumnal months; and almost every person of a phlegmatic temperament, who suffered the disease to take its course uncontrolled, was afflicted with ascites, or anasarca, or both. Great enlargement and induration of the spleen, with pain in that region, more particularly whilst exercising, were invariable concomitants. Occasionally a sense of fulness in the right hypochondriac region, and sometimes pain, extending into the right shoulder attended, with want of appetite and other symptoms of gastric derangement.

The modus operandi of constitutional derangement in producing the diseases under consideration, is yet a matter of doubt. A want of balance does certainly exist between the absorbent and exhalent systems; but whether it is owing to an increased action in the exhalents, or to a torpidity of the absorbents, or both; or how the local determinations above mentioned, produce the disease in some, although not predisposed, whilst others similarly situated, escape, are questions which necessarily arise. It is enough for us to know that they are symptomatic diseases. To treat them successfully, the loss of balance from which the effects, ascites and anasarca, arise, must be corrected before the disease can be permanently re-

moved. If some organ or organs are in a state of chronic inflammation, or of slight enlargement, with attendant functional derangement, our treatment should be directed accordingly, whilst at the same time we use the necessary means to remove the effects already produced. If we are successful in removing the cause and effects, by the same, or a combination of remedies, disease no longer exists.

Persons of a phlegmatic temperament appear to be most liable to enlarged states of the abdominal organs, which is probably owing, in some degree, to the relaxed condition of the vascular system, yielding to the unequal distribution of blood, which takes place in febrile diseases. In such cases, after the operation of cathartics, and the removal of the water, chalybeates are highly serviceable.

Being frequently unsuccessful with the common remedies employed in this disease, and supposing this want of success depended on the treatment being directed to remove a symptom rather than the disease itself; knowing, moreover, the inefficacy of most of the remedies used in removing the cause, and being fully convinced that calomel in large doses, possessed properties combined in no other article, to whose pervading influence every tissue of the human system yields submissively, and acknowledges its control, to it alone we look with confidence for the adjustment of that lost balance, and for the restoration of that harmony of action, which in each, and every part, or subsystem is so essential to the well-being of the whole. I Supposed, moreover, that if an active diuretic could be combined with it, and administered in such a manner that the functional darangement could be corrected, and the fluid discharged, before ptyalism was produced, that we would be in possession of a remedy to which we might resort with great confidence of success, in the most aggravated forms of dropsy.

Under these views the first case which presented was ascites, with œdema of the lower extremities, with no external marks of inflammation; an enlargement of the spleen with gastric derangement attended. It succeeded an attack of intermittent fever. Fifteen grains of calomel and four of squills, were ad-

ministered three times a day, which operated seven or eight times, daily, as a cathartic, producing bilious passages; and in seventy-two hours from the administration of the first dose, the hydropic collection was entirely removed, by the powerful diuretic and purgative effects of the compound. The carbonate of iron freely administered, combined with aromatics, prevented a relapse, and the recovery was rapid.

Being encouraged by the happy termination of the case above-mentioned, the same remedies were administered subsequently with equally good effects. And in every trial which was made I was more convinced of their superior efficacy, over every other plan of treatment recommended in such cases.

In cases where an inflammatory diathesis existed, relief was generally afforded by the calomel and squill as above directed, it having been rarely necessary to use more active depletory measures. Relapses, however, frequently occurred, and it was found necessary to resort to the medicines frequently, to give relief, and to correct the local affections which remained. During the intervals, counter irritants, laxatives, and the tinct. sanguinariæ canadensis were found beneficial. The last is a medicine which deserves more attention than it has received from the profession. My attention was particularly directed to its use by the recommendation given it by an eminent practitioner who considered it superior to every other remedy in diseases of the liver and spleen, calomel not excepted. Also by the frequent use made of it by the country people for the cure of intermittent fever, and enlarged spleen, which they call ague-cake.

Although I do not entertain so high an opinion of its virtues as the physician spoken of above, I know no remedy better calculated to relieve a patient with œdema of the lower extremities, and an enlargement of the spleen, than the saturated tincture of sanguinaria given in as large doses as the stomach will bear, three times a day; 3i. is a suitable dose for most persons.

The quantities of calomel and squills, above recommended, will not always have the desired effect upon the bowels. If

they fail to operate seven or eight times in twenty-four hours, the quantities should be increased—if they are too active, diminished—which, however, will seldom be found necessary. Active purgation seems greatly to enhance the diuretic properties of the compound. Some persons may suppose that the stomach would not retain the doses of squill recommended. In all the trials made with it, I have never seen it rejected. Even in five grain doses, it nauseates very little. Perhaps in the form of pills would be the most agreeable way to administer it.

It certainly would be an extravagant, and also an unwarrantable assertion, if I were to say that this, or any other plan of treatment will always be successful. But I do think that it is by far the most successful mode of treating ascites and anasarca. It is much more prompt and certain in its operation than the same medicines used in the common way; which if continued long without giving relief, as is often the case, generally salivates, an effect which I have never seen produced by the large doses of calomel recommended. When ptyalism does take place, it is injurious, because it prevents the continued use of the medicine without conferring any benefit whatever, never having seen any advantage obtained by salivating a patient in dropsy before the evacuation of the water.

Some practitioners use hydragogue cathartics daily, until relief is obtained, which probably will require ten or twelve active cathartics, and by their use the patient is so much exhausted, that a relapse almost invariably takes place, although tonics may be freely administered. The digitalis, as it is frequently found in the shops, is so insignificant a remedy, that it scarcely deserves notice. Having had but one case of hydrothorax, and that in the advanced stage, in the person of a sot, who had made a business of drinking spirituous liquors for twenty or thirty years, I am not able to say, whether the same success would attend this plan of treatment in that form of dropsy as in ascites and anasarca.

The squill that was used was dried in an iron mortar, placed near the fire, pulverized and confined closely in phials.

Art. VI.—*Cases of Mercurial Salivation, relieved by the Muriatic Acid.* By Alexander H. Peck, M. D. of Port Gibson, Mississippi.

[communicated in a letter to one of the editors.]

Sir—Having observed the remarks on the treatment of salivation in several of your last numbers, by different gentlemen, and with different remedies, I take the liberty of relating two cases that came under my treatment, of the malignant, dry, or concentrated form.

July 16, 1830. I visited a beautiful little girl, aet. 11, of the sanguineous temperament, and found her with a high grade of bilious fever which had been present two or three days, the exacerbations attending daily; one day without any perceptible abatement it would continue for twelve hours; the following day it would be characterized by a type of slight remissions: a livid spot upon either cheek appeared and disappeared during this day, and thus with all the general accompanying symptoms—rather full and quick pulse, burning skin, tongue coated with a brown fur, glossy eye and delirium, did the disease continue, in despite of a prompt and uninterrupted continuance of the febrile agents generally, for fifteen days.

29th. I had the satisfaction to find her apparently convalescent, and free from unnatural vascular excitement: the system was calm, and without prostration.

August 1. I extracted the bicuspis of the left superior maxilla which was hanging loose; it was fangless, owing to pressure above from the new tooth. Immediately in that space occupied by the tooth and extending to either side, a black speck was visible of about the size of one's finger nail, which seemed the effects of a salivation, although there was no flow of saliva, nor the slightest mercurial odour in her breath.

2d. The mercurial odour now present, the sudden extension of the gangrene, the enlargement of the cheek and face, half concealing the eye, placed the disease in its true light. The usual applications for ordinary cases of this disease were resor-

ted to; as strong a solution of the sulph. cupri as could be borne was applied; cold ablutions to the head generally; and 30 grains comp. pow. jalap ordered twice a day.

3d. The right cheek now as much swollen as the left was yesterday; the left eye nearly concealed; gangrene fast extending. I was told by the attendants that night not only brought on its accustomed glooms to the sick, but a burning fever to the little patient: prescription continued as before—the cathartic operating well, about three times a day.

4th. To-day she could not have been recognized by her nearest relatives, her whole head was so deformed by swelling, with great protrusion of the lips—complained of no local pain, but uttered a low moaning noise. A stench now attended too disgusting and offensive to be borne; a musquito bar was requisite to prevent the swarms of flies from their depredations, and she was evidently sinking as fast as the violence of the disease could carry her, without our having made the least arrest of its progression. The treatment being found wholly inadequate, was changed; not, however, till we had witnessed with the attending physician a case whose passing similitude to the one above was truly striking. It was a little girl 13 miles off, the type of whose fever and duration had been of the very nature of the one related—of the same age and temperament; the gangrene also had located in the same part of the mouth, and was also of the dry species. The only difference was, that her right cheek was gangrenous, whilst that now before us was confined exclusively to the left part of the mouth; and to render the similitude in the cases still more striking, the bicuspides in both had been removed and were found fangless. Commiserating the unhappy fate of these two little girls, we recollected curing a stubborn case of phagedena two months before, after many agents of the stimulating class had failed, with a strong solution of *muriatic acid*. It was resorted to—equal parts of rain water and of the acid being mixed. With a pair of scissors I exsected some sloughs of the gangrene from the roof of her mouth, which presented a greyish aspect within—the solution was insinuated with a feather into the portions exposed by ex-

section, and between the gangrene and the jaw-bone, which was partly bare. A solution of carb. sodæ was used as a counter-agent, when the acid should fall upon the healthy parts. The cathartic was continued as before: 60 grains of the compound were given during the day.

5th. The gangrene had extended so rapidly, that now, in five days from its commencement, it covered one half the mouth superiorly, and was bounded as follows, to-wit: from just behind the left incisor to the back of the farthest molar, and from the inside the cheek, which was now affected the size of a 25 cent piece, to the junction of the palatine and superior maxillary bones. The solution had been used some half a dozen times during the preceding day and night, causing little or no uneasiness, probably from not touching the vital portions: much larger sloughs were now removed from adjoining the alveolæ of the bone, and also from the inside of the cheek, which was now nigh perforated. The stimulating solution continued as before. Nocturnal fever reported to be increasing in violence with the other symptoms.

6th. The patient was enjoying a sound sleep at the time of my visit, which she had been long deprived of, nor had she experienced a moment's repose since early on the preceding day. I was told that after our departure on yesterday morning, an inflamed circle had appeared over the gangrene in the cheek, externally, which continued through the day, but at night disappeared; that the acid had been insinuated and applied freely to the disease throughout the day and early part of the night, when it caused such excruciating and even agonizing pain, that it was discontinued: the fever was attended with considerable delirium which kept her tossing from side to side of the bed, and, therefore, they had discontinued doing any thing more, "that she might die in as little pain as possible:" her cathartic was reported to have operated well. We left her without awaking her.

7th. On inspection found that the progress of the gangrene, if not arrested, was retarded: her head and face appeared rather diminished. The disappearing of the inflamed circle

and the present sensitiveness of the part, gave us strong hopes. The solution was diluted and continued; the cathartic operating well.

8th. The swelling of the right side of her face had disappeared: the now evident change in the aspect of the mouth, the healthy rising against the unhealthy portion, and the sprightly look of the little sufferer, produced sensations that are to be conceived but by him who has experienced them; her appetite was returning; the mercurial odour and her fever were retreating.

10th. She walked across the room several times to day without assistance; by this time too, her face had recovered pretty nearly its proper dimensions, and all appearances of salivation were ended. The part now presented a uniform, even, red surface: a portion of the alveola of the jaw containing three teeth, including the one removed, was found separated from the other parts, which was removed with a pair of forceps without the least difficulty a week or two afterwards, and left no deformity to the contour of her face.

To day we learned the fate of the other little girl whom we saw accidentally on the 4th. A few days previous, the gangrene had penetrated her cheek externally, which sloughed—another day, and her chin sloughed—yet another, and she sloughed into eternity!

Case 2nd. September 27th, 1830. I. J. son of A. J. æt. 8, of sanguineous temperament, residing on the high ground adjoining the swamp of Big Black River, was found labouring under a violent attack of congestive bilious fever which he had had for several days. The cerebral and abdominal vessels were alike congested, attended with almost constant delirium during the exacerbation; he was blooded, nauseated, blistered and the whole class of the ordinary febrifuge medicines resorted to; the mercurials taken, had to be followed with the most drastic cathartics, croton oil, &c.

October 3d. His system being greatly relieved of its morbid fulness and excitement, and being considerably better, a gangrenous spot was discovered inside his right cheek, which when

exsected left a cavity large enough to bury a large sized pea; directly against this, on the upper jaw another gangrene, about one inch and a half in extent was seen. Here again as much of the gangrene as could be conveniently, was removed. In respect to dryness of the mouth this case was also similar to the former. The muriatic acid was diluted to about one fifth acid to four parts of rain water in bulk. The application was used a day or two. On the fourth day after its commencement, we were told that not even a scar could be seen, so complete had been the relief.

Were we asked how this agent acts, we should draw our inference from the qualities of the successful remediate articles themselves, employed generally for this disease, which in our humble opinion give its nature and their modus operandi, to-wit: *That it is one of those local forms of inflammation exhibiting deficient action, a debility, if you please, in the vessels generally.* To discuss the different forms of inflammation, is not our province; every one is familiar with the two general grades, in common language, over and under action: The identical means most prompt and efficient for this latter order of inflammation, when applied to salivation, are attended with like success: in scalds and burns, which are also of the same kind, what is so adequate as the ol. terebinth.? Dr. Geddings tells us that salivation yields quickly to it also; and, medicated proportionally to the violence of the disease, in partial cases he has diluted it, while in others it required the entire strength of the application. Indeed on a reference to the many supposed specifics for this affection, we find those only of use which operate in this manner: among them may be enumerated sulphur. cold, porter, astringent gargles, charcoal, rhus glabrum, fumigations of cinnabar, emetics, ol. terebinth. &c. &c. The most useful of these will here be noticed.

The operation of cold applied for a certain length of time, is well known to give an exhilirating tonicity, if I may so speak, to any part of the system, particularly in low grades of inflammation; whether applied by ablutions, in ice or snow, or through the medium of cool air, the effect is identical.

Astringent gargles are the ordinary and every day applications, and are considered to act by constringing the vessels; remediate articles of almost every class, are changed in their effects as they are in proportion; for instance, one of the emetics may be made expectorant, diaphoretic, cathartic, emetic, or diuretic, according to the size of the dose. These mineral washes, when portioned delicately, are astringent: hence their utility in inflammation of the eye, in which they produce the most soothing sensations; but give their whole strength to the solution, as is customary in salivation, and a quick and almost lacerating pain is instantly felt.

The most conspicuous of the remainder that deserve a notice are emetics, which we are told have given marked relief in some cases. The well known virtues of this class, in all states of diseased or debilitated action of the system, (when however there is not general exhaustion,) in giving it a powerful tonicity need not be advocated here. Their action through the medium of the general system, produces precisely similar effects to those more locally resorted to; hence they are a stimulant to the debilitated salivary vessels.

To conclude; the decided, prompt and efficient relief afforded by the use of muriatic acid, one of the strongest of the stimuli, is but the verification of an adage, which is as applicable to medicine as politics, that the usefulness, or true test of a principle, is best established by the facility of its adaptation to extreme cases.

For an apology for dwelling upon two apparently isolated cases with such stress, we have to say, that in Warren county alone where these occurred, five cases came to our knowledge of this dry form of gangrenous salivation, in the space of a few months, all of which proved *mortal*—a tolerable indication of its general occurrence, and its usual termination.

Why the mercurial preparation evinced this particular grade in its effects, probably might be learned on reference to the identical cases. The subjects of this form, as far as our knowledge extends, were of the sanguineous temperament: great vascular action obtained in the type of fever: they were uncommon-

ly stubborn too; the state of excitement continued violent until its departure; the secretories were consequently above their proper secreting point: a scorching and almost vertical sun, from its uninterrupted course, had even destroyed vegetation in some places; from the departure then of febrile excitement, which was sudden, all the prerequisites for salivation in its most marked form prevailed. The system after having imbibed successive portions at every succeeding dose, by the time the excitement was allayed, had its mercurial capacity overburthened; the secretories were relaxed and rather below a healthy standard; great heat prevailed, a well known and important auxiliary; when diseased action was therefore directed to those glands, the onus became too great, and instead of a gentle, and ofttimes desired flow of saliva, nerves, muscles, ligaments and bones were destroyed.

ART. VII.—*Case in which a large number of Biliary Calculi were voided.* By Dr. W. D. JOURDAN, of Locust Shade, West Tennessee.

ON the 13th of Oct. 1830, Mrs. S. W. of Monroe, W. T. was attacked with what was supposed to be the *cramp colic*, to which she had been subject for more than ten years. A physician of that place was called in, who attended her until the 20th of the same month. On the 21st, being no better, and her physician absent, I was called upon to visit her, but owing to the distance at which I lived, (17 miles) and other engagements, I did not reach her house until 8 o'clock of the next morning, the 22d. Having examined her case I felt assured that colic was not her complaint, but that an uncommon derangement of the liver, even amounting to a complete obstruction of bile, was the cause of her disease. I drew this conclusion from the symptoms which had been, and were then present; such as an obstinate constipation of the bowels, a cramping pain arising

from the pit of the stomach, which gradually made its way to the region of the liver, after which it appeared to locate about the ducts leading from that organ, where it continued an acute pain; nausea now came on, followed by vomiting, which could not be checked; a general soreness in the region of the liver accompanied, with a sense of distension in this viscus; extreme languor followed, the pulse being all the time accelerated but little if at all.

About half past 8 o'clock on the 22d, I began my treatment. I gave her a portion of active cathartic medicine, every hour and a half until 3 o'clock next morning, when I changed these for those of a milder character, endeavoring all the while by various auxiliary means to effect a discharge from the bowels, for there had been none from the 18th, this being the 23d.

About half past 8, on the morning of the 23d, there was a small evacuation from the bowels, but not from the effect of medicine; in twenty-five minutes another followed apparently quite small and of the same character, seeming not to have been caused by what she had taken; nausea and vomiting, or rather perpetual efforts at vomiting still continued, in despite of all my efforts to relieve it. During the whole day very little was thrown up; in fact many efforts were made without any discharge whatever. About 2 P. M. she discharged in the act of vomiting twelve gall-stones of different sizes. The pain in the region of the liver now ceased immediately. In half an hour she discharged thirty-six by stool. During the 24th and 25th she continued to discharge them, feeling no pain, but being very weak, and much inclining to sleep. The whole number discharged was one hundred and twenty, weighing from one to seventy grains. She is still very weak, but is recovering fast, and it is believed will be hereafter healthy.

MISCELLANEOUS INTELLIGENCE.

Criteria by which a burn inflicted during life may be distinguished after death. (*Dr. Christison's Cases and Observations in Medical Jurisprudence.*)—1. What are the phenomena of vital reaction which appear immediately after the infliction of a burn during life, and remain after death? 2. Do these phenomena appear in every instance of severe burning, even when the person survives but a few minutes, or a single minute? 3. Can they be produced or imitated by any of the effects of burning produced immediately after life is extinct? Such are the points I propose to settle in the succeeding investigation.

1 and 2. Of the effects which follow the application of heat to the living body, the most immediate is a blush of redness to a considerable extent around the burnt part—removable by gentle pressure—disappearing in no long time—and not permanent after death.—Next to this in order, and occurring indeed most generally at the very same time, is a narrow line of deep redness, separated from the burnt part by a stripe of dead whiteness—bounded towards the white stripe by an abrupt line of demarcation—passing at its outer edge by insensible degrees into the diffuse blush already described—but not capable of being removed, like it, by moderate pressure. This line of redness may be seen very distinctly after the application of the actual cautery, the immediate effects of which represent exactly what may be looked for in a case of speedy death by burning. The redness is obviously caused either by extravasation or very minute capillary injection of the true skin. In every instance in which I have watched the effects of the actual cautery, as well as in the cases which have been observed at my request by others, it appeared in a very few seconds, sometimes in five, generally within fifteen, and once only so late as thirty seconds. I mean that in this short space of time, the inner edge of the redness surrounding the cauterized part was deep crimson, and incapable of being removed by pressure. I have farther examined carefully this appearance in the bodies of persons burnt a few hours before death, and never failed to observe it, forming a line on the entire skin near the burn from a quarter to half an inch in breadth, and about half an inch from the burn.—The next appearance in point

of order is blistering. I have not been able to determine the usual period at which blisters are formed. But from the observation I have made, it is obviously an uncertain consequence of a burn, if life be extinguished a few minutes afterwards. When the burning body is a scalding fluid, blisters generally appear in a very few minutes; yet sometimes in very extensive burns of this kind, especially in young children, there is no vesication at all even in many hours. When the burning cause is an incandescent body, vesication is by no means so invariable a consequence as might be supposed. For example, it is seldom witnessed at all round the edge of a burn produced by the actual cautery, probably on account of the circumscribed manner of applying the heat. At the same time, it is certainly often observed very soon after an ordinary burn, such as arises from the clothes catching fire; and the case of the woman in the second trial, proves that it may occur even when the individual is burnt to death on the spot, and consequently does not survive the burn many minutes.—The other vital consequences of burns ensue at too remote a period to be of any use for the object now in view.

3. It follows, then, that the only effects of burns which appear immediately after the injury, and remain in the dead body, are, *first*, a narrow line of redness near the burn, not removable by pressure, and, *secondly*, blisters filled with serum; that the former is an invariable effect; but that the latter is not always observable when death follows the burn in a few minutes.

Before these appearances can be assumed as indicating that the burn was inflicted during life, it remains to be inquired whether they can be produced or imitated immediately after death, while vitality still lingers in the body, or, to use Bichat's phrase, while organic vitality survives the extinction of animal life. For this end the following experiments were instituted partly by myself, partly by my friends at my request. In every instance the appearances are described from my personal inspection.

Experiment 1st.—In a stout young man who poisoned himself with laudanum, a very hot poker, and a stream of boiling water were applied to the skin of the chest and inside of the arm one hour after death. Next day no blisters or redness were visible on or near the burns. At the parts burnt with scalding water, the cuticle appeared as if ruffled, and could be very easily rubbed off; but there was not a trace of moisture on the true skin beneath. At the parts burnt with the poker

the whole thickness of the skin was dried up, brownish and translucent, but entirely free of redness or blistering on or around them.

Experiment 2d.—A stout young woman died in ten or twelve days of a low typhoid fever, and at her death was but little attenuated. Ten minutes after death boiling water was poured in a continuous stream on the breast and outside of one of the legs. The body was examined in a day and a half. On the leg no trace whatever could be discovered of the action of heat. On the breast, the place where the water had been poured on it was of a very pale brownish hue, the cuticle slightly shrivelled, dry, brittle and easily scratched off. The surface of the true skin below was dry; and around the burnt part there was not a vestige of redness or blistering. In this instance the heat was applied so soon after death, that the gentleman who applied it felt convinced he observed the chest heave up when the hot water was poured on it.

Experiment 3d.—A very powerful athletic young man poisoned himself with laudanum; and although the stomach pump was successfully applied not many hours after he had swallowed it, continued completely comatose, and without any sign of sensibility under the ordinary stimulants. Four hours before death a tin vessel, filled with boiling water, was closely applied on several parts of the arms; and a hot smoothing-iron was held on the outside of the hip-joint. Half an hour after death, a red-hot poker was applied to three places on the inside of the arm. The body was examined in thirty-eight hours.

Some of the spots burnt during life presented a uniform blister filled with serum. On two there was no blister; but the cuticle was gone, and the true skin dried into a reddish translucent membrane, at the edge of which there were drops of serum, and also particles of the same fluid dried by evaporation. Round all these spots there was more or less scarlet redness, particularly round the two spots last mentioned. A bright red border half an inch wide surrounded the whole burns; and the redness was not in the slightest degree diminished by firm pressure.—The spots burned after death, were some of them charred on the surface and not elevated; two presented vesications; but the blisters were filled with air, the cuticle over them was dry and cracked, and the surface of the true skin beneath was also quite dry. On the white parts of the skin there was no adjacent redness. At a part of the edge of two of the burns, however, the lividity which appeared on this, as on most dead bodies, approached very near the margin; but

the discoloration could be almost entirely removed by moderate pressure continued for a minute.

Experiment 4th.—Half an hour after amputation of the leg, a cauterizing iron was applied to it. Around the cauterized part whiteness and dryness were produced, but no redness or vesication.

Experiment 5th.—Two hours after death subsequent to amputation of the arm, a cauterizing iron was applied to the remaining arm. The appearances were the same as in the last experiment.

Experiment 6th.—Ten minutes after the amputation of a leg, a cauterizing iron was applied to it. The effects were the same as in the fourth experiment, except that blisters were formed round the burn—dry, however, and filled with air.

From these experiments it appears that the application of heat to the body even a few minutes only after death, cannot produce any of the signs of vital reaction formerly described. It farther appears that the lividity which follows death in most instances may assume such an arrangement as to imitate the red border produced by a burn during life. But an experienced person can easily recognize the appearance put on by lividity; and if its general appearance should not serve to characterize it, it may at once be known by the effect of continued moderate pressure in removing the redness. It should be understood then, that so far as the preceding experiments go, a line of redness near the burn, not removable by pressure, and likewise the formation of blisters filled with serum, are certain signs of a burn inflicted during life.—*Edinburgh Med. & Surg. Journal, April* 1831.

The Cholera in Moscow in a Letter from a Correspondent there. Moscow 30th Nov. 12th Dec. 1830.—The news of the disease having shown itself on the Russian frontier, bordering on Persia last year, attracted little attention. When, however, we were informed that the disease, after ravaging Tabris, the seat of the Persian government had spread to Teflis, the capital of Georgia, and thence to Orenburgh, and its government, Astracan, and, following the course of the Volga, leaving the colony of Sarepta untouched, (who are said to have saved themselves by a strict observance of the orders, cutting off all communication without the city,) had got to Saratoff, we began to think that it might visit us, which, as I have already informed you in my last, proved but too true. The disease showed itself unequivocally, though

sporadically, from the 12–24th to the 15–27th September last, and is pretty generally believed to have been imported from Saratoff, or the fair of Nishney Novogorod, though this is a subject which has not yet been sufficiently investigated, but shall be, if God spare me, as soon as I can command leisure. From this time it gradually, but not slowly, increased, showed a strongly-marked epidemic character, particularly affecting the lower classes of people, the ill-fed, ill-clothed, living in low and damp houses or cellars, and whose habits of intemperance and carelessness of health predisposed them to the action of the efficient cause, whatever that may be. About the 14–26th October there were, by the public accounts, 1066 sick of cholera; and on that day 244 were seized with the disease. It seems then to have been at its acme, and from that time has gradually declined. The number taken ill two or three days ago amounts to sixteen or seventeen daily; in consequence of this diminution, several of the public temporary hospitals have been shut up, and several others, of which that I have had the charge of is one, have not received patients for these last ten or twelve days. On the 5–17th November, the public accounts stood thus. From the appearance of the complaint till that day, were affected with the cholera 6531, recovered 1813, died 3508, remained sick 1210. The total number affected with the disease by the latest statement, December 7–19th, amounts to 7000, of whom more than 4000 died. About the time the complaint appeared in Moscow, the government thought it necessary to intercept all direct communication with the town; and no one was allowed to leave the city, or come into it without observing quarantine. The working classes to a considerable amount, were sent off to their villages, and many of the nobility left the city altogether, so that the population was considerably reduced.

The city is divided into twenty divisions, in each of which a temporary hospital was provided as quickly as might be; a medical council was formed, which met daily and frequently twice a day, at the General Governor's residence; and its members were formed principally from the inspectors of the hospitals, each of whom had under his charge or inspection several young surgeons and students. By the order of the minister of the interior, about sixty medical men were drawn from Moscow for the service of the interior before the complaint began, so that the inspectors had all the responsibility, and a great deal of the anxiety and fatigue on their shoulders; and among others I was one, since all the young men that assisted me had seen very little practice.

and, of course, were but little fitted to take charge of such cases, and such a disease as we had to do with. There were, in consequence, constant, and strong calls on us, both in the way of duty and humanity. In every quarter there is a police station, where some medical men are in attendance; and strict orders were issued, that, so soon as any one was taken ill in that quarter, information should be immediately sent to the police station, whence a medical man was immediately sent to examine and make his report. When symptoms of cholera appeared, he either immediately prescribed, or bled the patient, or, what we afterwards, in our quarter at least, found better, despatched him in a carriage with four horses, which stood yoked day and night, and conveyed him straight to the hospital. In this way as little time was lost as possible. From one cause or other, however, partly the ignorance and indifference of the lower classes, partly their dislike to the hospital, carriage, &c. we never got patients to the hospital of which I had charge sooner than six hours after the commencement of the disease, seldom so few as eight, and most frequently ten, twelve, eighteen, or more. In this manner two patients never entered the hospital, but died in the carriage that brought them, eleven before any medical assistance could be given them, and a great many in three hours, sometimes more, sometimes less. After they were brought to us several died in the warm bath, one of the first remedies we used; indeed, a very great number were in such a state, as to be out of the reach of medicine, being half dead, and without pulse at the wrist.

Under these and other disadvantages, such as the great press of business, the distance of the hospital from my house and from the apothecary's shop, and the time lost between the prescription and the exhibition of the medicine, it is no wonder, that in so rapid and formidable a disease as the Indian cholera, which it undoubtedly is, I should only have been able to save 56 out of 164, the number brought to us dead, dying, and very ill. We had no slight cases, though a *few* where the disease was less distinctly marked; and some where, the characteristic symptoms being quieted, the complaint appeared under the form of continued fever, with biliary irritation and derangement in the secretion of the bile.

I had studied what the Indian writers had written on cholera, as far as my library afforded me the means, and, through the Edinburgh Medical and Surgical Journal, knew their practice: but my notions on the disease were chiefly drawn from my own experience in bilious cho-

fera, and the close study for the greatest part of my life of the diseases of the digestive organs. I adopted the pathological views of this disease given by Dr. Ayre in his practical work on the disorders called bilious, which were confirmed by the perusal of that part of Dr. James Johnson's work on diseases of tropical climates relating to this disease, which I received about this time from England by Mr. Harvey. I determined, however, to act with caution, as having to do with a formidable foe, and to judge for myself unbiassed by any one, and trusting to my own knowledge and practical experience: and as bleeding had been so much recommended by the India writers, I thought it right to try it in three cases, all of whom died. Two were strong men. One, however, though naturally strong, enjoyed but indifferent health; the other two were bled under unfavorable circumstances, so that all the conclusion I could draw from these was, that, under such or similar circumstances, bleeding was likely to do more harm than good; and as at this period of the epidemic the surgeons at the police were in the habit of bleeding almost every one they were sent to, I found it necessary to put a stop to such indiscriminate practice. Local bleeding I frequently found useful, and general bleeding, in one case particularly, was of great use in the course of this disease, where it had the appearance of abdominal inflammation; in another, a bleeding, employed early in the disease, and done effectually, that is, fourteen or sixteen ounces taken from the arm, followed by magnesia and rhubarb, cut short the complaint, which the patient, one of the young men of the establishment, had temporized with by using opium and oil of peppermint. The third day after the bleeding, he was at his duty again, and has remained well since.

Founding my practice on the pathological views which I have alluded to, I used a modified Indian practice, seeing clearly that unless I could make some strong impression on my patients at the first, the exhibition of medicine was in vain. A *medicina expectans* in such a state of things is worse than trifling; and having frequently experienced the good effects of twenty-grain doses of calomel in high excitement of the frame, I had no fear of giving such a dose to those who had no pulse at the wrist, so that my invariable practice was at first, till we got a vapour bath fitted up, to strip the patient so soon as he was brought to the hospital, and put him, cold and shrunk, into the warm bath, of which the temperature was gradually increased as high as he could well bear. The bath was made more stimulating by adding a

large proportion of common salt. While the bath was getting ready, we administered a glass of good brandy punch; and when in the bath, watching a favorable moment when we thought the stomach was least likely to reject it, gave him twenty grains of calomel, mixed with as much gum-arabic, and washed it down as the Indians do, by from ten to twenty drops of oil of peppermint, mixed with an ounce and a half of pure water, and, in general, forty drops of laudanum of the Edinburgh Pharmacopœia, or instead of this, the calomel was washed down by a table-spoonful of Cogniac, and forty drops of laudanum, according to circumstances. This, or a diminished dose, was repeated according to circumstances a few hours afterwards. I rarely had occasion to give a third dose, as by the time we would have given it, the patient either showed morbid symptoms, or an abatement of the more urgent ones. It was then that I employed, what I consider a very essential part of the treatment of this disease, the regular and free, but prudent use of purgatives, so as to follow up and assist the effects which I suppose to have been produced on the digestive organs by the calomel already given for this purpose. I have sometimes given an emulsion, with castor-oil and laudanum; but having found it often returned by vomiting, I generally used an infusion of senna, with manna and Epsom salts, unless where I feared inflammation; and in such cases I preferred the emulsion with castor-oil; in other cases these, though taken in free doses, were not sufficient to open the gall-ducts, so that I was forced to use something more stimulating, such as pills of rhubarb with calomel, or the compound extract of colocynth of Reuss's Pharmacopœia with calomel. By the use of one or other of these medicines, I commonly succeeded in procuring a free flow of morbid bile from the liver and gall-bladder, which I believed and found by experience, to be the surest key to unlock the disease; for I lost no patient (excepting one where an infusion of senna was given injudiciously, and by mistake) in whom I succeeded in procuring a free biliary discharge by stool once The watery stools and vomiting subdued, they never returned, unless the patient had a relapse by his own imprudence in eating. One of our young students, who entered on his office in bad health, was seized with the disease. By active treatment and great care, I left him in bed at the hospital at 10 at night, out of danger, after having been two days very ill. The next morning, on visiting him, I found him without pulse. On inquiry as to the cause of this sudden, and hitherto inexperienced change, I was informed that, feeling himself dispo-

sed to eat soon after I left him the preceding evening, he had eat freely of some soup, and a fowl of which it was made. The quantity he took was more than his stomach could bear; brought on vomiting and purging, and such a collapse of the vital powers as defied all medicine. He sunk in spite of all my efforts to save him. This is not a solitary case. I am making preparation to try the effects of galvanism, electricity, and the protoxide of azote, or a considerable proportion of oxygen with atmospheric air. It appears to me probable that the primary impression of the poison which produces the disease is made through the organs of respiration on the blood itself, which appears to me to be unduly carbonized, and hence unfitted for the natural excitement of the nervous and muscular organs. Hence the morbid symptoms of diminished irritability of the heart, and undue influence of the brain on the muscles in general, as well as the torpor of the senses.

The *cordon sanative* has been removed thirty versts from Moscow.

Dec. 7–19th. By the last accounts the number affected with cholera exceeds 7000, and the number of deaths about 4000.—*Ibid.*

On a New Practice in Acute and Chronic Rheumatism. By J. K. Mitchell, M. D. *one of the attending Physicians of the Pennsylvania Hospital.*—In the autumn of 1827, a patient affected with caries of the spine, was suddenly attacked with all the usual symptoms of acute rheumatism of the lower extremities. One ankle, and the knee of the opposite leg tumefied, red, hot, and painful, afforded as fair a specimen of that disease in its acute stage as is usually met with. The usual treatment by leeches, purgatives, and cooling diaphoretics, with evaporating lotions, had the effect of transferring the symptoms to the other ankle and knee, and finally to the hip. Disappointed in the treatment, I began to suspect that the cause of the irritation might lie in the affected spine. The difficulty of cure, the transfer of irritation from one part of the lower extremities to another, without any sensible diminution of disease, and the fact of the existence of caries in the lumbar vertebræ, which lie near the origin of the nerves of the lower extremities, rendered probable the opinion, that in the spinal marrow lay the cause of this apparently indomitable and migratory inflammation. Under this impression, I caused leeches to be applied to the lumbar curve, and followed them by a blister, placed on the same spot. Relief promptly followed these remedies, and the pain ceasing to be felt in the limbs, was perceived only in the immediate vicinity of the spin-

al curve. After the blistered surface recovered its cuticle, a few leeches placed over the diseased spine removed the pain, and left the patient in the usual state of indifferent health attendant on such forms of spinal disease.

Striking as were the benefits of the applications made to the spine in a case of apparent inflammatory rheumatism, they did not lead my mind at the time, to the general conclusions which, viewing the case as I now do, they ought to have suggested.

In the beginning of the ensuing winter, another case of a similar kind presented itself. A little female patient, having curvature of the cervical vertebræ, was attacked in the night with severe pain in the wrist, attended with redness, tumefaction, and heat. As on the appearance of these symptoms, the pain in the neck, to which she was accustomed, subsided, I easily persuaded myself of the spinal origin of this inflammation, and accordingly applied leeches to the cervical spine, with the effect of procuring a prompt solution of the disease of the wrist.

This case led me very naturally to the reflection that, perhaps other cases of rheumatism might have an origination in the *medulla spinalis*, and depend on an irritation of that important organ. In the following spring an opportunity of testing by practice the truth of this opinion presented itself. William Curran, a respectable livery stable keeper in Marshall's Court, had been for upwards of two years afflicted with a rheumatism of the lower extremities, which gradually deprived him of the use of his limbs, and finally confined him to his chamber. Regular medical aid, and many empirical remedies had been procured, without an abatement of the pain, which became at length almost intolerable.

On my first visit I found him in his room, in a paroxysm of pain. His legs were swollen from knee to ankle, and the enlargement of the periosteum and integuments, gave to the anterior face of the tibia an unnatural prominence. In that place the pain and tenderness on pressure, were particularly developed. He was also suffering severely from pain in the scalp, which had existed for a short time previously, and was at length almost insupportable. Along with these symptoms appeared the usual febrile action with its concomitants.

Notwithstanding the significant hints given by the spine-cases referred to, I treated this case for a time in the usual manner—depleted freely, purged actively, blistered the head, and having caused an abatement of fever, administered corrosive sublimate and decoction of sarsa-

parilla. Defeated in all my efforts, I at length suggested to my patient the possibility that his disease was so unmanageable because we had not applied our remedies to the true seat of disease, and that by addressing our measures to the spine, success might yet be found. Accordingly on the 16th of February, 1828, nine days after my first visit, I had him *cupped at the back of the neck*, and as he could not bear any more direct depletion, inserted a large seton over the *lumbar spinal region*. The cupping, followed by blisters to the back of the neck, relieved his head, and as soon as the seton began to suppurate freely, his legs became more comfortable. From the 25th, nine days after the insertion of the seton, I visited him but seldom, although I had seen him once or twice a day until that period. Indeed, I paid him but seven visits after the 25th. The last was on the 30th of March. Soon afterwards he resumed his usual pursuits, and about the beginning of June the seton was removed. Since that time he has not had a return of his complaint, and is at the date of this paper, in the full and vigorous exercise of all his physical faculties.

I could scarcely doubt as to the cause of the cure in this case, because the treatment applied to the spine was that alone which had not already been fully and fairly tried, either by me or those who had preceded me. Indeed, the last applications were made with some hope of success, and the grounds of that hope were expressed to the patient, who was fully persuaded that the spinal treatment was the chief, if not the sole agent of restoration.

No other well-marked case of rheumatism presented itself in my private circle of practice, until in the winter of 1830 Mr. Teale's work on neuralgic diseases reached this country and began to attract towards the spinal marrow a greater share of medical attention. Although in his essay, I found nothing *directly* calculated to sustain me in the opinion I felt disposed to adopt concerning the spinal origin of rheumatism, I rose from its perusal with increased confidence in that opinion, and resolved experimentally to examine its truth. The first *well-marked* case of simple inflammatory rheumatism which subsequently presented itself, was the following.

Robert Gordon, well known as the carrier of Poulson's Daily Advertiser, fifty-six years of age, of vigorous constitution and active habits, was the subject of the attack. Observing a severe pain in his right heel and ankle, immediately followed by redness, heat and tumefaction, he caused himself to be largely bled and took some salts and mag-

nesia. On the following day the pain and swelling increased, and the ankle and knee of the opposite limb becoming similarly affected, he was confined to bed.

On the 3d day my first visit was made. The patient had then a full, strong, frequent pulse, flushed face, dry skin, whitened tongue, and complained much of the severity of the pain in his legs, and of his incapacity of enduring the slightest pressure or motion. As he had already been purged and had used a lotion, I directed the application of seventeen cups to the lumbar region, so as to abstract twelve or sixteen ounces of blood.

Next morning found the pain almost entirely gone, does not complain of moderate pressure, and is able to move his legs without inconvenience. Ordered a draught of salts and magnesia, with an evaporating lotion of camphor in alcohol.

3d day. Pain in legs scarcely perceptible, but the shoulders, elbows and wrists, are beginning to exhibit marks of severe inflammation, expressed by pain, tumefaction, heat and redness. Ordered twelve cups to the cervical spine.

4th day. The patient sits up, complains of stiffness, but no pain except in one wrist, and that very slight. Directed Epsom salt and magnesia.

5th day. Finding nothing for which to prescribe, arranged the patient's diet, recommended the occasional use of aperients, and took leave of the case.

Called on the 10th to enquire into results, and found that there had been no return of disease.

Since that time a very severe winter has passed, during which the subject of this report has continued in his customary health, and in the pursuit of his usual employments.

The reader will, in the above case perceive, that the general bleeding, though very copious, proved of no service, and that the large local depletion of the lumbar region, benefitted solely that part of the disease which lay at the peripheral extremities of the nerves, supplied by the lower end of the spinal marrow. The inflammation in the upper extremities continued afterwards in progress, and was arrested only when cups were placed over the cervical end of the spinal column.

The whole case exhibits a fine exemplification of the difference in the character and extent of the influence of general and topical depletion, and proves that *local blood-letting is most potent when applied to*

that part of the spine, which supplies with nerves the parts in a state of active inflammation.

As I feel, in common with the profession, a greater confidence in Hospital reports, especially when made by those who are not by interest or reputation blinded or misled, I shall present the history of some cases treated after the new method, as drawn up at my request, by Dr. Stewardson and Dr. Norris, the resident physicians of the Pennsylvania Hospital.

(Here follow eight cases of Rheumatism treated in a similar manner, with speedy relief in most of them, and benefit in all.)

Although other cases might be cited in confirmation of the views here taken, I have not leisure at this time to digest and arrange them. At no very distant period I hope to be able to bring the subject more fully before the profession. I may observe in general, that, as far as I now recollect, only two cases of apparent rheumatism, have in my hands, either in private practice, or in the Pennsylvania Hospital, resisted the treatment recommended in this paper, and both of *them* were in reality *neuralgia*, and exhibited no traces of inflammation. One of them was an affection severely painful, located in the bottom of the heel, the other was gastric and intercostal.

The preference given to local depletion over other local measures, arose from the greater apparent success and promptness of its action, which scarcely left any thing to be desired: but cases will occur in which other measures must be used, and in which, perhaps, all measures will fail. We are warranted, however, in declaring our conviction, that few failures will happen in thus treating *acute* rheumatism, and that success will diminish, as passing through *chronic* rheumatism, we enter on the ground of *neuralgia*, a disease which sometimes spontaneously disappears; but is scarcely ever, in this city, cured by merely *medical* means. The art of the surgeon occasionally subdues it, and the physician often allays, but seldom removes it. Being paroxysmal, and often slumbering for weeks or months, it is not unfrequently mastered in appearance, though seldom cured in reality.

Amer. Jour. Med. Sciences, May, 1831.

Hepatic and splenic derangement, simulating organic disease of the heart or aneurism of the aorta. By J. Franklin Vaughan, M. D. *Physician to the Alms-house of New Castle County, at Wilmington, Delaware.*—A married lady, who had generally enjoyed good health, and was the mother of several fine children, was attacked in the latter part of July, 1825, with bilious fever, then prevalent in the district in which she resided. The attack being violent, and the fever of a highly inflammatory type, her physician, (as he informed me,) had to resort to active and decisive measures for relief. Depletion free and repeated, by the lancet, active purging, and all the usual antiphlogistic means were put in requisition. In the course of a short time the fever yielded, tonics were prescribed, the patient pronounced convalescent, and medical attendance discontinued.

In about six weeks, however, the doctor was summoned to visit his patient in haste, as she was "dangerously ill." He found her labouring under an urgent sense of suffocation, violent palpitation of, and a most distressing feeling of weight about the heart, with acute pain. Venesection was at once resorted to, and some relief afforded, but the pain in the region of the heart and the palpitation continued. A consultation with an aged and respectable practitioner was held, and her disease was pronounced to be either, (*which*, they found it impossible to ascertain,) "an organic affection of the heart, or an aneurism of the aorta at its arch." Under these circumstances, a palliative course was adopted, consisting of venesection once or twice a week, according to the violence of the symptoms, with digitalis, absolute rest, low diet, anodynes at night, &c. This plan was continued for near two months, without any alleviation of the distressing symptoms, but, on the contrary, (as the patient stated,) they were all greatly aggravated, when I was called to see her.

Her situation at this time was truly deplorable; *the palpitation of the heart was so violent as to throw up the bed clothes,* (at every diastole,) *so as to be distinctly seen across a large room!* and the mental despondency was greater than I had ever seen it, despair seeming literally stamped upon the features. This state of mind, however, I was not much surprised at, when I learned from the lady that she had been made acquainted with the fearful diagnosis and still more terrible prognostic—a sudden death. The patient was, as might be expected, very much debilitated; her skin was sallow and unhealthy, the bowels torpid, being evacuated only by medicine, and the pulse wiry or corded, quick and frequent.

After a minute enquiry into the history of the case, and a most careful examination of the thorax, both by mediate and immediate *auscultation*, I was induced to believe that there was really no primary or permanent disease of either heart or aorta. Some other cause for the situation of the patient was then to be sought for, and my attention was immediately directed, (by the history of the case and the generally depraved state of the system,) to the chylopoietic apparatus; and here I soon found a state of things which confirmed the opinion already expressed—a very great enlargement and induration of the spleen, some enlargement, evident induration, and slight tenderness on pressure in the liver, and the usual debility and disorder of stomach consequent to biliary obstruction.

Might not then all her sufferings be referred to the derangement of the functions of these important organs, and all the indications of disease in the thorax be sympathetic and delusive?

In the subsequent consultation, this opinion was looked upon, (by those in attendance previously,) as fanciful or "speculative;" and the indications, or methodus medendi predicated upon it, positively condemned as being utterly inadequate to relieve the urgent and alarming symptoms, and only calculated to hasten the termination of an incurable disease! On, however, fairly, plainly, and explicitly stating the case in the presence of all concerned, my opinion, and the treatment founded upon it were adopted—because it afforded some hope, the other none.

The course pursued may be related in a few words. A very slight salivation was excited by the blue pill, and maintained for about four weeks; an epispastic applied over the liver and spleen, dressed, (as soon as it could be borne,) by the ungt. merc. a light but more nutritious diet was allowed, and cheerful society, &c. &c. recommended. Soon after a gentle ptyalism was established, the obstructed secretions were restored, the torpid bowels became regular, and the enlargement of the liver and spleen were found to be yielding to it and the counter-irritants; a perceptible diminution in the violence of the palpitation, with relief of pain, weight, oppression, &c. directly followed, and the general health gradually and regularly improved, until a complete restoration was effected. During the convalescence, tonics, (especially cort. peruv. with rad. valerian.) were freely administered, aided by a nutritious diet, and as soon as the strength was sufficiently increased, active exercise, especially riding, &c. &c. Five years have now elapsed, and this lady continues perfectly well.—*Ibid.*

Of the Uva Ursi in Nephritic Cases, &c.—Medical practitioners seem to agree, that the Uva Ursi (which Linnæus makes a *Species* under his *Genus* Arbutus) abates the pains and shortens the fits of nephritic and calculous attacks; notwithstanding the best opinions are that it has no power whatever as a *solvent* either of stone or gravel. Hence doubts have arisen how this fact can be explained; and the solution of this question seems connected with practice.

A short conjecture, therefore, shall be offered on this subject; namely, that the benefit arises from a power in the uva ursi to remove morbid action in the urinary organs; and that a healthy condition in these organs prevents the *aggravations*, which might otherwise attend diseases belonging to the urinary system. Several *parallel cases* may be supposed to occur in medical practice. For example: an ophthalmia may be much relieved by the removal of particles of dirt accidentally lodged within the eyelids; and the member to which the prepuce is attached, may in like manner profit if the prepuce be cleansed from foul matter collected underneath it. Perhaps it may strengthen our conjecture, if we add to it another *hypothesis*, founded on the conceived utility of *astringents* in the cases in question. The Swedes and Russians are said to use the uva ursi largely in tanning; which proves this article to contain much of the *tannin principle;* and if an affinity is thus found to exist in the uva ursi as to certain *dead* animal matter, the same affinity may be held to exist as to certain *living* animal matter; so as to yield to the patient the virtues of a very powerful astringent, supposing astringents to be useful in these cases.

This, it is true is all conjecture and hypothesis, but it is at least not of an unnatural description. It also agrees with the fact of the reduction of the *pulse*, which *finally* occurs after taking the uva ursi; besides receiving some confirmation from the details of the following *case* which has recently occurred, and which, in truth, has led the writer of this article to the present discussion.

An elderly person, occasionally attacked with gout, observed his urine to become suddenly and largely mixed with blood, and yet he experienced no pain whatever on the occasion, nor did he feel any while passing the urine; but when the stream issued along the urethra, it was stopped for a time, in each instance, by *large* clots of blood. The patient (who in general had good health, and had suffered nothing from strains or other violence) thought this not improbably owing to a gouty affection; and suffered it to proceed without much attempt to

check it, during two whole days; notwithstanding the forebodings of some medical writers. The delay seemed to have its hazard lessened, as (from the absence of all local symptoms elsewhere) he judged that the discharge of the blood proceeded from vessels seated in the *bladder*. He employed, therefore, merely rest and abstinence for the moment, with an open body; and refrained from cordial liquors; using for a part of his drink, barley water in which gum arabic was dissolved. He had his views, nevertheless, constantly fixed on uva ursi, digitalis, *astringents*, and such other measures as the case might demand. After about forty-eight hours, however, he began to take moderately of the uva ursi, prepared by boiling half an ounce of the powdered leaves, for ten minutes, in a pint of water; each dose amounting *nearly* to a very small wineglassful. The result was, that in two or three days the urine became free from fresh blood; and nothing seemed left behind but *small* clots of blood, with a little *fine gravel*, which again went off in about two days; the uva ursi being still, for a little time, continued.

Two or three days after this supposed cure, and after leaving off the uva ursi, the urine became what some have called *muco-purulent*. Upon a fresh resort, however, to the uva ursi, the urine became once more bright, and gravel was no longer to be seen in it.

As *diet* may be suspected to have had some concern here, we shall state the following particulars regarding the diet; what is not mentioned on this point having been conducted on principles of old established habits.—The patient on the occurrence of slight *gouty* symptoms, had left off mild barreled cider (which he had been using with great moderation,) and substituted weak brandy and water, for a part of his dinner beverage. But his urine becoming paler and rather more in quantity than usual, he returned to his cider. His urine in a few days becoming bloody, the cider was omitted, and, after this symptom abated, was replaced by a little "*stout*" beer, manufactured in London, which, though it seemed sound, abounded in loose *mephitic* air. This was drank during two days, and suddenly during two days, the *muco-purulent* appearance in the urine occurred. On giving up the beer, and returning to the uva ursi, the urine soon again became clear. After drinking weak brandy and water, and weak wine and water, by turns the use of beer and of cider has once more been introduced into the diet, as occasion served, in small quantities, without inconvenience. From this statement we may suspect that the

bloody urine occurred from *incidental* causes, disconnected with diet; and that the muco-purulent appearance in the urine was solely owing to *pus* produced during the healing of ruptured bloodvessels, with which *mucus* mixed itself. Be that as it may, the uva ursi appears not to have been misplaced in either stage of the business; and the probable benefit of it will be farther confirmed by an important remark to be made hereafter.

But a new circumstance is now to be noticed. Dr. Thomas, in his Modern Practice of Physic (as edited in this country by Dr. Hosack,) informs us, that "a case of *hæmaturia* is recorded (in the 8th Vol. of Medical Facts and Observations,) which had resisted repeated bleedings and warm bathing, salivation, emetics of different kinds, camphor and opium in large doses, *uva ursi*, *mephitic* alkaline water, &c.; which was *quickly* and *effectually* removed by giving the patient a pint a day of decoction of *peach-leaves*. The decoction was made by boiling an ounce of dried leaves of the peach tree (amygdala Persica Linn.) in a quart of water, till it was reduced to a pint and a half." So far Dr. Thomas.—The patient, in the case above related, on this hint, joined about thirty drops of excellently *distilled* peach-water to his uva ursi, and thought he found the addition useful, but certainly not injurious.

We shall now conclude with some brief remarks.

1st. It seems improper to employ either uva ursi, or the peach leaf, beyond necessity; as it is not every medicine which can be made either habitual in the human system, or prophylactic. In the case of the gouty patient above detailed, no opium was requisite to allay either pain or irritability, or to procure sleep; and it may be questioned, therefore, when opium has been given with the uva ursi, whether the opium was not called for by too liberal an employment of the uva ursi. And this leads to the following rule; namely, never to use the uva ursi except when wanted; but, when wanted, to adopt it fearlessly, as being possibly the only resource offering. A similar remark may be made as to the peach leaf; particularly as it is an article so acceptable to some, as to encourage them in taking more profuse and frequent doses of it than may be prudent.—2d. We ought, in every event, to employ the uva ursi while any clots of blood or particles of gravel remain to be removed; since these (as appeared with our gouty patient) seem readily got rid of under the use of the uva ursi. Were this use of it neglected, it might here happen (as in the case of *other* foreign bodies accidentally lodged in the bladder) that a coat of calculous matter might

soon *surround* these substances with a small but regular mass of *stone*, which time might increase; and as the uva ursi *cannot be depended upon for dissolving* stone, this might produce serious evil. In this remark it is, that we find our encouragement for using the uva ursi, while any thing improper remains in the bladder, which the uva ursi can seem likely to contribute to expel.—3d. Though we have supposed above, that the uva ursi only prevents an *aggravation* of symptoms, yet it may be queried whether a disordered state of the urinary organs may not sometimes *of itself* provoke nephritic or calculous attacks; and thus introduce these as *primary* affections.—We may now, lastly, pass on to the following observation, but merely as a subject demanding further inquiry. Some appear to have conceived that gout and calculous complaints have a species of relation to each other; not, indeed, a relation of *sympathies* but a sort of *watchfulness over each other*, in order that both should not appear at the same time in the system. And this, indeed, is in some degree implied in the case of Dr. Barton, of Philadelphia, who is said to have had nephritic symptoms *alternating* with gout in his feet.—This is a hint which it may be useful sometimes to bear in mind; and it is repeated, that as such only it is offered.

On the whole, we may conclude, as to the uva ursi, that it is an important, if not an unrivalled resource in the hands of medical practitioners, in nephritic and calculous cases; as it is apparently, also, for keeping the urinary organs in a healthy condition: and if either, and still more if both these positions be true, we must admit that its uses may be very essential, in certain cases, to the *whole human system*. With regard to peach leaves, we must wait the result of further experience.—*Boston Med. & Surg. Jour. No. 12, Vol. 4.*

Sulphate of Copper in Hemoptisis.—The sulphate of copper acts with great power on the extreme vessels, operating as an astringent, and causing them to contract, when preternaturally dilated, and is consequently a very useful remedy in checking hemorrhages. As an illustration of the correctness of this principle, I will state some cases which have come under my observation.

An old man was suffering from profuse hemorrhage from the nose. When I first saw him, he had bled more or less freely every day for more than a week. Two physicians had previously seen him, and prescribed some local applications, which had produced a temporary sus-

pension of the bleeding. He was at this time pale and much exhausted, so much so that I was apprehensive he would die, if not soon relieved; for the blood from the nose was almost a stream, and the state of the bed-clothes and the utensils around him bore testimony to the large quantity he had lost. Finding local applications to be of no avail, I determined to give an emetic of sulphate of copper. The effect was remarkable. So soon as the patient became nauseated by the medicine, the bleeding stopped, and he had no more trouble from it. I have repeatedly directed the same medicine in cases of epistaxis, after local applications have failed, and in no instance have I been disappointed in its operation.

A man was suffering from severe hemoptysis. His cough was incessant, and had been so for many hours previous to my visiting him; his expectoration consisted of fresh and florid blood alone. I immediately directed him an emetic of the sulphate of copper, in small and repeated doses. The effect was as striking as in the case of epistaxis. So soon as the patient was nauseated, the expectoration of blood was diminished; and by the time vomiting was induced, it was entirely checked. This, man, in a few weeks, died of phthisis; but he had no more trouble from hemoptysis.

Hemorrhages from the uterus, when dependent upon a relaxed state of the vessels, can be often checked by the use of this medicine. Hence it has been found very efficacious in cases of profuse menstruation arising from this cause; and also in excessive flowing after delivery, when the common local applications have failed.

The sulphate of copper should always be given in solution. When prescribed with a view of checking hemorrhage, the dose should be from two to five grains, repeated every five minutes, in order that nausea may be maintained for a time before free vomiting is induced. When the intention is to relieve the stomach, the dose should be from ten to twenty grains.

In thus speaking of the utility of sulphate of copper in cases of hemorrhage, I do not mean to underrate other remedies for the same affection. I am aware of the power of acetate of lead, and I do not know but that other emetics, besides the one above mentioned, may produce the same effects. I only speak of what I know that this article is capable of doing. It has the recommendation over most emetics, in producing but little debility and I have not been disposed to relinquish it for other articles with whose virtues I am less acquainted.

Ibid. No. 3, *Vol.* 1.

Intermittent Hemiplegia. From the Medico-Chirurg. Review.—Dr. Elliotson has put on record, in one of his clinical lectures, a case of this kind, which is the first he has ever witnessed. Our readers are aware that we have recorded several cases of this description on our pages. We insert the present case for several reasons.

"There was a case of disease of the nervous system presented of a curious character, the first of the kind I ever met with—intermittent palsy. I have read of it in authors, and you will find it mentioned by Cullen as *paralysis intermittens*. Now among all the patients I have ever seen, and these amount to between thirty and forty thousand, including those in various public establishments and private practice, I had never met with an instance of this description. It was a case of intermittent hemiplegia. The man was admitted into Jacob's Ward some time ago, and I mentioned his admission at the time. I gave him no medicine because I was desirous of seeing whether his account was true or not. I seldom give medicine in aguish or intermittent complaints, till some one in the hospital has witnessed the occurrence of the paroxysms. He staid here three weeks without having a paroxysm. He was, however, a very respectable man, and I did not doubt his account. He then went out of the hospital, enjoined by me to return if his disease reappeared. One day when I came to the hospital, some time afterwards, I found him in the courts, and he said he had been seized with a paroxysm that morning, and he actually was then in a state of hemiplegia of the left side. I saw it myself. I made him walk, and he dragged his leg in a semi-circular way, as patients usually do when they are laboring under hemiplegia, and he could not raise his left arm. It began at 10 o'clock, and this was the usual course of the disease. He had told me originally that the paroxysms came on at 10 o'clock in the morning, not every day, but every third or fourth day, and, with a single exception, never after a longer period than that; but on one occasion, there was an interval of sixteen days. He was 48 years of age, and had been subject to this affection for two years and a half; and the paroxysm would last from three to four hours. But although it only lasted that time, he was not perfectly clear from it through the whole of the day. He never knew the paroxysms begin later than 11 o'clock, or earlier than 10; from 10 to 11 was the regular period, till a week before he had been admitted, when one attack came on at half past 10 in the evening—the usual hour, but in the evening instead of the morning. The affection was

not more frequent then than when it first began. The man looked sickly, as if he had had ague, but still more as if he had suffered from a hot climate; and it appeared that he had been in the East and West Indies, and that he had had fever both at Bombay and Batavia. He had suffered from dysentery, and when he was in the hospital he had diarrhœa. I do not doubt that this was the effect of malaria—that his hemiplegia was a form of ague. I will not quarrel about words—you might say it was not ague, because unattended by shivering, fever, or sweating; but I have no doubt it was as much the effect of malaria as ague is: it was merely a variety of the same affection of the system. Supposing this to be the case, and having witnessed a paroxysm myself, I now gave him the sulphate of quinine, and, as the disease was of long standing, I began with a good quantity—five grains every six hours: this medicine very soon put a stop to the complaint, but not till I had increased the dose to ten grains every six hours, so that he took forty grains in the twenty-four hours. This is the dose that is often required in quartan ague, and the present was a worse form of the disease than quartan, because it occurred on the third or fourth day; and the longer the attacks, the greater is the difficulty of curing the affection, which may be considered as so much the more of a chronic character. It is not a matter of wonder that that large quantity was required. He continued in the hospital from his first admission, on the 13th of October, till the 23d of December—rather more than three months—without any other attack whatever, and his health became greatly improved. It is wrong to suppose that malaria does nothing more than produce these particular forms of intermittent disease; it poisons the whole body, and many persons are destroyed by it who never had ague at all, so deadly is the poison. His health, however, regularly improved under the quinine; he became strong, his countenance was better, and altogether he found that he had received very great benefit. However on the 28th of the same month, five days after his presentation, he came to me, saying that he had a slight attack, a very slight one, that morning, but still it was an attack, and it occurred rather later than usual, some little time after 11 o'clock. When I saw him, at about half past one o'clock, it was then nearly gone off. I increased the quantity of sulphate of quinine to fifteen grains every six hours, and if that be not sufficient I shall give him more, as he is to come to me from time to time. I had a person in the hospital who was not cured of ague with less than a scruple every six hours, and

therefore I shall not be surprised if that quantity be required in the case of this man; but I have no doubt that eventually he will be perfectly cured, though he may need very large doses.

"This is a very interesting case, proving that paralysis is not *necessarily* an *organic* affection; that hemiplegia does not necessarily arise from effusion, or from compression of any kind, at least of an organic nature. If any compression do occur in this man, it can only be during the fit, for at other times he is perfectly well; it is entirely I presume an affair of *function* induced by a particular poison. I have at this moment, in private practice, a very curious case, in which disease has arisen from malaria; it has occurred in a young gentleman about eleven years of age, who lives by the side of the Thames. He had diarrhœa at school, which was allowed to run on; he was, however, taken home, and treated very properly by the gentleman who attended the family, by leeches to the abdomen, and I believe a blister, and all went on very well. He had tenderness just on one side of the umbilicus; he was, however, seized all at once, at a certain hour of the evening, with violent irritation, severe itching, tingling, and redness at the leech-bites, some feverishness, just at the very part where all the leeches had been applied, and every leech-bite became red and swollen. His sufferings were extreme, but, after lasting for a certain time, all these symptoms went away. At the same hour the following evening, the same thing occurred, the leech-bites became swelled and hot, and he fell into a state of general excitement, from, as it would appear, the itching and tingling. The medical gentleman immediately saw him, and thought the attack was of an aguish character; and, as the family lived in a low spot by the side of the Thames, he gave this lad twenty grains of sulphate of quinine, in divided doses, before the time of the next expected paroxysm. The attack came on the next evening, but at a later period than usual, showing that the remedy had produced an impression. It is common for the remedy not to stop the disease at once, but cause the fits to be postponed. I, however, was sent for, and told the family that I was quite satisfied that the youth was going on right; that the quinine was the only remedy, and that it must be persevered in at the same doses. The fits were very distressing—indeed, to the family, alarming, and we both agreed that it was better to go on with twenty grains in the twenty-four hours. The next day the paroxysms appeared later and more slightly, and then came on once in two or three days, and still more slightly; he presently became perfectly well.

At the end of a month he went out of doors, and was exposed to cold, and from his extreme anxiety to regain the time he had lost from school, for he was a fine boy, a paroxysm came on again, but rather mildly; the medicine was again had recourse to, and the immediate effect was a postponement and alleviation of the next paroxysm, and I have no doubt that if he continue to take the remedy for some weeks, he will not have a relapse. These remedies will not cure the disease unless you give them for some time after the disease has appeared to cease. Sometimes it is necessary to give them for many weeks; sometimes it is necessary to do more than this—to remove the patient from the spot. Just as in syphilis, if a person get cured, and return to the same quarters, the mercury he has taken will, of course not prevent him from again catching the disease; so a person may be cured of ague, but if he continue to live in the same unhealthy quarters, of course the poison may operate afresh upon him; and as, in syphilis, mercury must be taken for some time after all the symptoms have disappeared, so must quinine *after* ague."

We have seen many cases where paralysis was evidently not the effect of organic disease, but of malaria. One, which we are not at liberty to mention by name, but which we lately saw with Dr. Paris (who will confirm the truth of this statement,) is still more remarkable than the case recorded by Dr. Elliotson. A young gentleman, the son of one of the most PUBLIC CHARACTERS in England, travelled, more enthusiastically than wisely, through Italy, during the summer of 1830. The consequence was, a malaria fever, after which he lost the power of *all* the voluntary muscles of the body, from head to foot! In this deplorable condition, he was conveyed from Venice to London, *like a corpse in perfect health*. All the functions went on regularly—intellectual and organic—but all animal power of motion and locomotion was lost. This general paralysis continued for some weeks after his arrival here; and Dr. Paris very judiciously abstained from violent remedies, such as strychnine, &c. trusting to the recuperative powers of Nature. Motion gradually returned—first in the upper, and afterwards in the lower extremities—and we believe he is now walking about, and enjoying good health. Such accidents, though seldom on so remarkable a scale, are every year happening in Italy and other malarious countries, though they are very imperfectly known to the profession of Great Britain.—*Ibid. No.* 16. *Vol.* 4.

Stethoscopic Signs of Pregnancy.—In another part of this number, we have referred to this mode of diagnosis, and the subject will be still further illustrated by the following notice of the experiments of Dr. Ferguson, of Dublin, for which we are indebted to the N. A. Med. Journal of the present month. (April, 1831.)

Dr. Ferguson has published an account of his success in detecting pregnancy by means of the stethoscope. He says he has never in a single instance been mistaken: and in one instance only of undoubted pregnancy, was unable to detect the stethoscopic signs. Two distinct sounds may be detected; one arising from the pulsations of the fœtal heart, the second from the placenta, which Dr. F. refers to the passage of blood in the arteries connecting the placenta to the uterus. The placentary noise may generally be found in one of the iliac regions, and in the same subject is always found in the same place. It may be mistaken for the pulsations of the iliac arteries, but the last will be heard perhaps in all cases on both sides, and can only be perceived in the groin, while the noise produced by the placenta is heard over some space, perhaps three or four inches square. The fœtal heart may often be heard in every region of the abdomen. Although it and the placenta are sometimes heard on the same side, and even in the same spot, yet usually they are on opposite sides; the fœtal heart is generally perceived in one of the iliac regions; but, unlike the placenta, it is not always heard in the same place, in the same individual—but it does not vary much from the point where first heard. Its double beat is well marked; and the frequency of its pulsations is, says Dr. F., always much greater, often double that of the mother's.

In the Dublin Hospital Reports, this subject is also treated of by Mr. Kennedy, who agrees with Dr. Ferguson, that the placental thrill is owing to the transmission of blood from the arteries of the mother to the placenta. He appears, however, to consider this thrill as important in detecting pregnancy in its early stages, before quickening, and before the pulsation of the heart can be recognized. He has noticed the placental thrill as early as the 10th week, and hence, in many cases, this diagnostic mark of pregnancy becomes extremely valuable—cases to which particular reference is unnecessary.

[*Ibid. No.* 11, *Vol.* 4.

Large doses of Ipecacuanha in Dysentery.—Mr. W. Twining, in a sensible paper inserted in a late number of the London Medical Ga-

zette, strongly recommends the employment of "ipecacuanha in very large doses, for the purpose of relieving tenesmus and irritation, restoring a healthy state of the alvine evacuations, and in more remote stages of the disease, promoting the healing of intesti al ulcers."

Mr. T. acknowledges that ipecacuanha has long been employed in the cure of intestinal profluvia, and that its utility in alleviating some of the symptoms attending those diseases seems, to a certain extent, generally admitted. But he adds, that the mode in which it is usually exhibited induces him to believe that no great reliance has been of late years placed in this remedy, although it was originally introduced to the notice of the profession in Europe, as a medicine of the greatest efficacy in the cure of dysentery. "A reference to the history of the remedy may satisfy us that it has been held in estimation in the cure of that disease, just in proportion as it was administered in large doses; and it was formerly sometimes prepared by boiling, so that the emetic properties of the root were destroyed; whereas, when given in small doses, it has been considered of less efficacy, and in severe cases is now generally looked on as an adjuvant of minor importance, the benefits appearing to belong, in a great measure, to the other remedies with which it is combined."

Mr. T. recommends the remedy to be given, combined with a bitter extract, whereby vomiting is counteracted. "I have generally," he remarks, "used the gentian for this purpose, and by prescribing these two remedies in nearly equal proportions, we may, if requisite, give from six to twelve grains of ipecacuanha without often exciting nausea." "The remedy appears advisable in the early stage of almost all ordinary cases of acute dysentery; in the treatment of which the first object, (after adequate bleeding) is to clear out the bowels by a dose of castor oil; twelve hours after this has operated freely, the patient should take six grains of powder of ipecacuanha, with four grains of extract of gentian, and five grains of pil. hydrarg. in three pills, which are to be repeated every night at bedtime, and at daylight in the morning; a dose of comp. pow. of jalap (dr. i.) is given daily at 11 o'clock in the forenoon, but if the patient be of a very irritable habit, dr. ss. will be sufficient for a dose. By these means we may soon procure a cessation of all the distressing symptoms, and it is then sufficient to continue six grs. of ipecacuanha, with four of the extract of gentian every night, giving a smaller dose of comp. pow. jalap, or a moderate dose of castor oil, every morning, for 4 or 5 days more."—*N. A. M. & S. J. No.* 22.

ART. VIII.—*Remarks on a Review, in the May number of the Am. Jour. of Med. Sciences, of "A Treatise of Pathology and Therapeutics,* by JOHN E. COOKE, M. D., &c." By the author of the Treatise.

A TREATISE of Pathology and Therapeutics, published by the writer of the following observations in 1828, and for sale at Carey, Lea and Carey's bookstore in Philadelphia since that time, has been noticed in the last number of the American Journal of the Medical Sciences, (No. XV. May, 1831,) it is presumed from the signature, by one of the collaborators of that periodical. It is proposed to make some remarks upon that review in this journal; and although they may not be as extensively circulated, and will not even be presented to the same readers, as the review, yet it is the best that can at this time be done to set the views of the author in a correct light. If the Editor of the Am. Jour. of Med. Sciences should consider them fit to publish in his periodical, his sense of justice will, it is presumed from his reply to the application made to him to publish an answer to the review, cause him to give them a place in his next number.

The Reviewer, after an introduction complimentary to the West, proceeds thus.

"The treatise opens with some judicious observations on the proper method of pursuing medical investigations, in order to arrive at just and permanent principles; and the author next expatiates on the causes which have retarded the progress of the science. The great cause of this evil he attributes to the extreme proneness of physicians to frame hypothetical theories from partial and limited premises, instead of tracing up the varied phenomena of disease through their chain of causes to the first or remote cause of the derangement, in the true spirit of the inductive philosophy. After such an explicit avowal of views which we have been taught to consider as the only sound procedure for the attainment of a correct system of pathology, aided, however, by a knowledge of the laws of physiology

* The review came to hand while the writer of these remarks was confined to his bed, and in consequence of his indisposition this paper could not be prepared in time to take its proper place among the original communications.

and autopsic inspection, we did not expect to find the author straightway propounding a theory of his own, which we fear will be found to rest on no better data than he had the page before deprecated. This theory is no further novel than in the extensive application made of it to explain the production of the multiform characters of disease, but as it constitutes the key-stone of the system of pathology, which this treatise is devoted to verify and enforce, we shall give it in the author's own words, that the reader may be fairly put in possession of the aim and tendencies of the work under consideration."

With this view the reviewer then proceeds to give an extract from the preface to the treatise. This however the author cannot admit as giving a fair view of his work. That part of the preface given in the review was written to show, not the doctrine of the treatise, but merely the manner in which he was led on to investigate the leading points of that doctrine. And although these leading points are mentioned in the preface, they are barely mentioned; and he is persuaded that every man who takes the trouble to read the two pages of the preface given in the review, and compare them with the whole tenor of the work, will say, that this extract does not "put the reader fairly in possession of the aim and tendencies of the work under consideration."

The assertion contained in the above quotation from the review, that the author has straightway propounded a theory of his own, and the intimation, that he has departed from the proposed plan of investigating the subject, the author thinks far from being well founded.

The first chapter of the work is occupied by a notice of the theories of various deservedly celebrated writers, and an attempt to point out the manner in which the investigation should be conducted. In the 10th and 11th sections it is said,

"The remote causes of disease, operating on the human system produce uniform effects. This must be the consequence of their operating in an uniform manner, and of the system's acting, under their influence, uniformly according to certain laws by which it is governed. The exposition of this manner and of these laws, or a statement of the immediate effects of the remote causes on the system, and of the changes wrought in it in consequence of these effects, will show the whole connexion between the agents ascertained to be the remote causes of disease, and their effects the symptoms; and is the true theory.

"In the following pages I shall attempt the investigation of this connexion, or, *a statement of the immediate effects of the remote causes on the system, and of the changes wrought in it in consequence of these effects.*

The course which, it is believed, the inquiry must necessarily take to be successful, is stated in a few observations which follow."

The sum of these observations is given in section 18 in the following words.

"Applying these manifest truths and inferences to the phenomena of disease, the remote causes of fever, and the whole train of symptoms proceeding from them, denominated a febrile paroxysm, with all the intermediate links, constitute a series of causes and effects: &c."

In section 19 an inference is drawn from section 18, in the following words.

"Hence it is evident that, in order thoroughly to investigate *the connexion between the remote causes and the symptoms of disease*, we may inquire into the effects of the causes ascertained; next into the effects of those effects, or new causes; and so on, descending until we arrive at the ultimate effects, the symptoms in question; or, that we may inquire into the causes of the symptoms, and into the causes of those causes, and so on, ascending until we arrive at the remote causes; or, that these two modes may be combined, and that the agreement of the two in the same result is a strong confirmation of its truth."

After showing at some length that this mode of investigation had not been followed by the authors of the theories that have been promulgated, the chapter concludes in these words.

"I shall now attempt the investigation of this connexion by inquiring into the effects of the remote causes; next into the effects of these effects, or new causes; and so on descending to the ultimate effects, the symptoms in question."

After such an explicit avowal of views which we too have been taught to consider as the only sound procedure for the attainment of a correct system of pathology, it would indeed have been extraordinary to find the author departing from the plan proposed and straightway propounding a theory of his own. But he cannot agree with the reviewer, that this has been done "*in the work under consideration.*" On the contrary, the plan proposed has been most rigidly adhered to, it is believed, throughout the whole. It is not intended to say that the book contains no theory. On the contrary, the absolute necessity of a knowledge of the true theory or explanation of the operation of the remote causes of disease in producing their effects, in order to scientific and successful practice, is strongly urged. It is a hypothetical theory of which the reviewer speaks, and which the writer has in view here. The work in question is devoted to the explanation or theory of the operation of the remote causes of fever.

But this explanation may rest on supposition or conjecture, as have many explanations or theories of disease. It was the intention of the author to avoid this entirely; and rigidly to pursue the inductive method of philosophising.

The second chapter commences the investigation on the plan laid down in the first. After showing in the first three sections, that the action of the heart is measured by the force of the pulsation of the arteries, and by the temperature and colour of the surface and extremities; and stating, in the next section certain agents which have been considered remote causes of fever, the author lays down the proposition, (section 85) that "*all the remote causes of disease directly or indirectly weaken the action of the heart*," without propounding any theory, or intimating to what this proposition is to lead; and in the following sections endeavours to show that this is true of each remote cause separately.

In sections 86–88 it is shown that abstinence from food is a remote cause of fever, and produces weakened action of the heart.

In sections 89–90, the same is shown to be true of fatigue.

In section 91, the same is shown of external violence from a blow or a fall; in 92, the same is shown of the depressing passions; in 93, of want of rest or sleep; in 95–96, of every kind of stimulant used to excess; in 97, of strong tea and coffee; in 98, of intense application of the mind; and in 99 &c. of cold.

The 3rd, 4th, 5th, and 6th chapters consist of an argument, rigidly upon the inductive plan, to show that a dense gas the product of the putrefaction of vegetable matter is the remote cause of hot weather endemics. Several doctrines are noticed in the course of this investigation, and among others Dr. Ferguson's, at length.

In the 7th chapter, section 595, it is shown that "weakened action of the heart is the consequence of respiring the dense gas arising during the putrefaction of vegetable matter."

In the 8th chapter, the winter epidemics, called bilious pleurisy, &c are shown to be the effect of the joint operation of the same dense gas, and of cold.

The chapters 10 to 16, inclusive, contain arguments to show that contagion is not the remote cause of plague, yellow fever, or typhus, but that these diseases arise in precisely the same circumstances in which the annual hot weather endemics of this continent arise: and that the doctrine, that a general epidemic constitution of the atmosphere

is the cause of the diseases of hot weather, is unfounded. In these chapters the evidence is drawn almost entirely from writers who advocate the doctrine of the contagious nature of these diseases.

In the 17th chapter, section 1204, the following is given as the sum of the argument.

"We have now seen that all the known remote causes of fever weaken the action of the heart, and that weakened action of the heart precedes every fever. (691*) Weakened action of the heart is therefore one link of the chain of causes extending from the remote causes to the symptoms of fever; and all those causes which produce this link of the chain, are themselves remote causes of fever; inasmuch as all those causes which produce it, are remote causes of its effects."

After a few inferences from this section respecting some other remote causes of fever, occupying less than a page, the volume closes; and the author ventures to say that a man might read the whole and have no conception of the theory to follow.† He has not time to read over the whole work to ascertain the fact, but is confident that in the whole seventeen chapters of the first volume, there is no part of the theory advanced but what has been mentioned; viz. that the remote causes of fever directly or indirectly weaken the action of the heart, and that weakened action of the heart precedes all fevers. The last words of the first volume are, "We next proceed to inquire into the effects of weakened action of the heart:" and with this inquiry the second volume opens.

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*The following references in support of the last clause of this sentence are given at section 691.

Boerhaave's Practical Aphorisms, Eng. translation, 563.
Cullen's First Lines, xxxiv, xlvi.
Darwin's Theory of Fever, Supplement, 1. 1. 6.
Rush's Works, vol. 3, p. 3, 4.

†In the winter of 1827 the manuscript of the work in question was delivered, with occasional remarks and some omissions, as a course of lectures, the first delivered by the author in Lexington. A number of the intelligent and inquisitive of the class were struck with the novelty of the course pursued, and visited the writer and were very urgent to know what doctrine or theory he had in view—what was to be the end of the discussion they were daily hearing, of principles, the tendency of which they could not perceive. Their inquiries were for some time evaded; they were told to be satisfied to proceed step by step, and inquire whether the successive points discussed were established, or not; so that they might be unbiassed by preconceived notions, when they should discover the conclusions resulting from the doctrines advanced. These visits were however repeated, and at length the visiters became so importunate in their requests, that when the winter was well advanced, the writer gave them a sketch of the whole.

In the first section of the second volume the question is proposed. What is the effect of this cause, weakened action of the heart?

In the next section, it is answered: One consequence immediately flowing from weakened action of the heart, is the diminution of the quantity of blood sent into the arteries; whence follows weakness of the pulse, a very obvious consequence of the diminution of the power which distends the arteries; paleness and coldnes of the surface, consequences, as certain, of the failure of the usual supply of blood to the surface; and diminished bulk of the external parts, shrinking of the features, and shrivelling of the skin, all necessarily following deficient fulness of the cutaneous and subcutaneous vessels.

In the following sections, 1211 to 1218, it is shown that another consequence of weakened action of the heart inevitably following in every case, is an accumulation of blood in the great veins entering into the heart; and in the sections following, it is shown that this accumulation cannot be confined to the vena cava, but must extend into the large branches of that vein which proceed from the head, the liver, the spleen, and stomach and bowels, including the hæmorrhoidal veins, the veins from the kidneys, and the veins from the pelvis including the uterine branches of the internal iliac veins.

This being established, section 1248 runs thus;

"Accumulation of blood in the venous cavity" (the vena cava and branches above-mentioned) "being the effect of weakened action of the heart, is another link of the chain of causes extending from the remote causes to the symptoms. The next question is, What are the effects of this cause, accumulation of blood in the venous cavity?"

Chapter XIX is taken up with the answer to this question. It is there shown that accumulation of blood in the vena cava and its branches produces various effects; viz. enlargement of some parts, disturbance of the functions of the head, the stomach, the liver, the kidneys, and the uterine system; which it is unnecessary further to notice at this time, as the object now is to show not the truth or correctness of the various arguments or explanations, but that the proposed plan of investigation is followed. The writer has also in that chapter attempted to show that the alternations of low and high action in fever are the consequence of the operation of sudden accumulation of blood in the vena cava, upon the heart, exciting it to action too great to continue and therefore after some hours falling back into weakened action; thus allowing of a gradual return of the accumulation of blood in the

vena cava, &c. ready, as soon as the heart becomes capable of it, to excite it again to increased action; again to subside into weak action: to be followed by accumulation, and increased action: the whole to be repeated as long as the heart can be thus excited; which, observation shows, is rarely more than twenty days, often not more than fourteen: though sometimes for months.

The twentieth chapter contains the mode of practice founded upon the doctrines advanced in the preceding part of the work. It is not intended to say that the mode of practice laid down is entirely different from that formerly practised by the writer, as well as others, in every case; but that the principles upon which it rests, the reason for the different prescriptions, as well as the object to be held steadily in view, are rendered, at least to the writer, and to many with whom he has had communication, more clear and more intelligible; so that the practice is to them simpler, plainer, and more satisfactory. There are some cases, too, in which the practice of the writer and many of his friends, has been entirely changed, in consequence of the inferences drawn from the doctrines of the work in question. Thus, in menorrhagia, and in threatened abortion, the practice is so opposite to all preconceived notions on the subject, that the writer has always had a difficulty in inducing patients to submit, except when they had previously acquired entire confidence in him—and yet the practice has been altogether successful up to the present time.

The remaining chapters of the second volume are occupied with the application of the general principles in the first twenty chapters to various chronic affections.

It is evident from this account, if it be a correct one, that the author has not straightway propounded a theory such as he has reprobated, but that he has endeavoured to adhere to the mode of investigation proposed. If he has failed in this, he is willing to admit it as soon as he is made sensible of it. Until then he must necessarily rest in the belief that he has not built upon hypothesis, but that what he has advanced consists of facts and fair inferences from them.

The reviewer after giving the extract from the preface of the treatise above-mentioned, proceeds to object that the theory of the author "is much too mechanical for general reception in this age of physiological and vital pathology."

The author had been taught to believe that "the inductive philosophy" is "the only sound method of procedure for the attainment of a

correct system of pathology," and therefore that whatever conclusion this leads to, should be received, without respect to the pre-judgments of any age.

The reviewer says,

"It is no where shown that the liver is actually in a state of congestion, or that the vena cava and its branches seriously suffer from an accumulation of blood in the precursory stage of fevers, though it must be admitted that such accumulation to a certain extent constantly takes place during the continuance of rigors, from whatever cause induced: consequently we are constrained to class this theory, according to the author's own definition, among those hypotheses which have been so long the bane of medicine."

It is shown in sections 1218, 1220, 1229, that accumulation or congestion of blood in the vena cava and the liver is the inevitable consequence of weakened action of the heart, which always occurs in the forming stage of fever; it is known from the manifest enlargement of the liver and spleen in the living body, as well as from dissections of such as have died in the cold stage of fever, that this state of the vena cava and the liver exists; and the reviewer himself in the very passage quoted says, "it must be admitted that such accumulation to a certain extent constantly takes place during the continuance of the rigors, from whatever cause induced;" and *yet he concludes*, "consequently we are constrained to place this theory among those hypotheses which have been so long the bane of medicine."

Speaking of the endeavours of the author to prove that all the remote causes are either directly or indirectly debilitating, the reviewer says;

"On this point we would remark, that whatever may be the mode of operation of these causes, it cannot be denied that fever does not take place until a general or local irritation is set up, which is, indeed, itself a state of fever, whenever the irritation is sufficiently intense to affect the rest of the system."

This seems to the writer to amount nearly to the following. Fever does not take place until fever is set up; the substitution of the word *fever*, for *irritation*, being entirely admissible, if irritation be, as the reviewer says, a state of fever whenever it is sufficiently intense to affect the whole system. If this be incorrect, the passage is not understood.

The reviewer proceeds:

"Besides, the view taken of the operation of the remote causes, necessarily supposes fevers to be idiopathic and general affections, a doctrine fast vanishing from the minds of physicians," &c.

Must the inductive method give way because it leads to conclusions inconsistent with the fashionable doctrines of the day? The reviewer further says;

"We will merely observe in passing, that if it can be shown, as we believe it can be, that the fevers heretofore esteemed idiopathic arise from local irritations, and are nearly allied with phlegmasial affections, that then whatever may be the nature of the remote causes inducing them, that these causes must be either directly or indirectly essentially stimulating in their operation, a conclusion in contravention of the author's theory."

The forepart of this quotation seems to the writer, to admit that the doctrine, that the fevers called idiopathic arise from local irritation nearly allied to phlegmasia, is not yet established; and the writer fully believes never will be. After this admission, it seems to the writer to be taking rather more ground than yet belongs to the reviewer, to draw his "conclusion in contravention of the author's theory."

The reviewer next proceeds to examine a little in detail the author's explanation of the mode of action of some of the more frequent of the remote causes of fever. He "admits that the first effect of abstinence is not only weakened action of the heart, but of all the powers and functions of the system. This state of depression, however, is not a state of fever; on the contrary, it is universally considered one of our most efficient means to remove or alleviate an existing febrile affection."

This state of depression is not said by the author to be a state of fever. Such a state sometimes is not followed by fever, though it generally is. The question however arises, does not this admitted weakened action of the heart cause necessarily an accumulation of blood in the veins entering into the heart? Where else can the blood be accumulated, when the heart is sending on a quantity less than usual? What are the effects of this accumulation? manifestly it must produce increased action whenever the heart becomes capable of it, under the continually increasing quantity of blood pressing into it. And if the heart be incapable of it, the load must continue in the veins, and produce such effects upon the parts concerned, as such an accumulation is capable of producing.

As to the objection, that the depressing operation of abstinence is universally considered as one of our most efficient means of removing or alleviating an existing febrile affection, the writer thinks it is destitute of force. The very same might with equal propriety be made against admitting cold to be a remote cause of fever; which the

reviewer does not question. Of the depressing effects produced by cold it might be said with equal propriety, "This state of depression is not a state of fever; on the contrary, it is universally considered one of our most efficient means to remove or alleviate an existing febrile affection." True, the depression is not a state of fever; but it leads to a febrile state; and when fever is produced, as it very often is, the state of depression is called the cold stage of the fever. And, notwithstanding that cold is universally and justly considered as one of our most effectual means of removing or alleviating an existing febrile affection, yet is it undoubtedly a remote cause of fever; and the reviewer admits it. These reasons, therefore, for denying that abstinence is also a remote cause, are not valid.

The Reviewer proceeds to say;

"When abstinence is carried to the extent of producing disease, it does so not by weakening the action of the heart, which effect rather wards off for a time the evil, but by changing the character and qualities of the circulatory fluids, and rendering them so acrid and irritating as to produce not only most intense inflammation of the stomach and intestines, but also of other tissues of the system, which, when they prove fatal, destroy life amidst unexampled pain and suffering," &c.

Does the reviewer seriously offer this explanation as one founded upon observation and the inductive method of philosophising? Has he produced any evidence of this acrid and irritating state of the circulating fluids? Has he any to produce? The writer knows persons who suffer excessively from abstinence but for a few hours. Is their blood already in this acrid and irritating state? On eating a few mouthfuls, the pain is removed. Is this by removing the acrid and irritating state of the circulating fluids? The writer has known the painful sensations arising from abstinence to go off in a few hours, though the abstinence was continued, and the person abstaining the whole day, to rest in the latter part of it entirely free from pain. The case stated by Dr. Currie of Liverpool, in his Medical Reports, is entirely opposed to the Reviewer's opinion on this subject.

A gentleman was prevented by a scirrhous tumour of the œsophagus from swallowing. The difficulty gradually increased with the increase of the obstruction. From October 17th "he was able to swallow only a tablespoonful of liquid at a time, and with long intervals. It was with difficulty that he got down seven or eight spoonfuls of strong soup in a day, and this quantity gradually diminished. On the first of November, the passage seemed wholly obstructed." After

hat he continued to swallow two or three tablespoonfuls of milk laily, till about the 15th November; but though it rested in the œsoohagus for some time, it was constantly ejected at last; and for the ast twenty days of his life he made no attempt to swallow. After hat time an attempt was made to sustain him by the regular adminisration of clysters of broth. The result of this remarkable case was, hat until Dec. 1st "no man had ever perhaps approached death by steps more easy." (p. 212.) This was fifteen days after he had deisted from making any attempt to swallow. On the first of Decemer, however, a change took place. "On the morning of the 1st Decemer it" (the pulse which had for a month previously been healthy,) became small and frequent; and still more frequent after the delirium commenced; the state of the pulse, as well as almost all the other symptoms from that time forth, very nearly resembled the symptoms of the last stage of fever, when it terminates fatally."

The reviewer denies that famine is a remote cause of fever on the ground that a famine in France in 1817, produced certain chronic diseases, but that there was a remarkable exemption from febrile diseases. The author has, however, equally good testimony to show that famine has produced epidemic disease. Russel, in his history of the plague at Aleppo in 1760, 61, and 62, says, (p. 9) "Through the summer of the year 1757, grain of every kind bore a very high price, and as the winter approached, became scarcer and dearer; insomuch, that from the month of December in that year, till the following June, most parts of Syria and Mesopotamia might be said to have suffered all the miseries of extreme famine. In the month of February, 1758, a malignant petechial fever made its appearance at Aleppo, and advancing rapidly with the spring, raged throughout the summer and part of the autumn. This fever reigned with influence not less extensive than the famine, and both together produced every where a mortality little inferior to that of the true plague."

Here then is evidence on both sides; but with this material difference. That of the Reviewer is negative; that of the author positive. The former proves only that in the case specified, famine did not produce fever. The latter proves that fever did accompany famine; and the capability of an agent to produce certain effects being shown in one instance proves that agent to be one of the causes of those effects. Indeed, it would be no difficult matter to multiply proofs that famine produces fever. Numerous instances might be cited of famine in pla-

ces besieged being followed by pestilential fevers; and these have ever been considered as so inseparable, that if famine prevail, pestilence is confidently expected shortly to make its appearance.

Moreover, the testimony produced by the Reviewer is not only inferior in character, but it is by no means inconsistent with that produced by the author. An agent universally admitted to be a remote cause of fever may and often does produce *chronic* and not *acute* epidemic disease. Of this the occurrences on board of the Channel Fleet of England in the years 1794, 95 and 96, afforded a striking example. In 1794 an epidemic fever prevailed on board the fleet, in 1795 an epidemic scurvy, and in 1796 there were numerous cases of both scurvy and fever among the crews of the ships.*

The reviewer thinks that when fever is produced by excessive bodily exertion or by stimulant drinks, that it is during the excitement and hurry of the circulation. This the author would not call a state of fever. The fact is, that febrile excitement frequently follows the debility which the Reviewer admits to be often consequent on excessive bodily exercise; as well as the debility left after a night of intemperance. This however, is a matter respecting which every man must form his own opinion from his own observation.

The Reviewer says; (p. 129)

"In making contusions from external violence, as falls and blows, the cause of fevers, the author has drawn his argument from the analogy of their first effects to the concussions of the brain from the same causes."

The author is not aware of having done this. The whole he has said on the subject is given in section 91, as follows.

"External violence from a blow or a fall produces fever and various morbid symptoms, besides the local symptoms from contusion. Blows have the effect of reducing the action of the heart in a remarkable manner. A violent stroke, not on the head only, but on any part of the *body*, will in an instant stop the action of the heart. Falls have the same effect precisely, it being immaterial whether a body be impelled against a man, or he against the body. I have seen a person who had fallen from a horse continue for some time with a feeble pulse, pale countenance, and yawning and stretching, as in the cold stage of fever."

The author did not intend in this passage to represent *contusion* as the cause of fever. He has seen fever follow the state of debility produced by a fall, when there was no contusion properly speaking. He has had a case of nephritis produced by a fall from a horse in which

* See Trotter's Medicina Nautica vol. i.

there was not the slightest appearance of contusion, and the patient it is believed lit upon her feet.

Neither does the author, in making blows and falls, remote causes of fever, draw his argument from analogy, &c. He merely states the fact that they do produce fever; as may be seen by referring to the first sentence of the quotation given above. Argument in such a case is superfluous. Every man who has not yet made up his mind on this subject, must do so, if at all, from observation. The whole passage above quoted, after the first sentence, is occupied with showing that blows and falls weaken the action of the heart—and in support of this, reference is made to observations made by the author; but no argument is used.

The author rests upon the alleged matter of fact for the truth of his theory, so far as there stated. The reviewer in objecting, combats alleged matter of fact, not by an appeal to observation, but to an argument founded upon a hypothesis, in which a case is supposed that was not in the view of the author at all when he wrote the passage in question; viz. a case of contusion in which the part is so injured as to become "the seat of irritation and fluxion." So far from having such in view, the author expressly excepts that case in the first sentence of the quotation; and the tenor of the remainder of it, accords with that meaning. He will allow, however, that another word instead of *besides* might have conveyed his idea more clearly to the mind of the reviewer. The passage might have been expressed thus. "External violence from a blow or a fall produces fever and various morbid symptoms, *independently of*, or *over and above* the local symptoms proceeding from contusion." Nevertheless the word *besides* does certainly bear the same signification with the other words used above; and that this was the intention and meaning of the author is evident from the tenor of the subsequent part of the passage, from the express declaration in section 1858, latter clause, and is well known to all those who have heard his views at large.

The Reviewer says; (p. 130)

"Cold is one of the most constant and evident causes of disease, and the author endeavours to prove, chiefly from the facts furnished by Currie, that it invariably does so by weakening the action of the heart."

This is a singular misapprehension of the passage, it appears to the writer. The author in speaking of cold, does not, as he had done in the case of the previously mentioned causes, say any thing in the

commencement of his remarks on cold, of its being a remote cause of fever. He enters immediately upon the subject of its power to reduce the action of the heart. This he endeavours to show by reference to the well known experiments of Currie, to those made by Dr. Stork of Bristol, and Drs. Spooner and M'Donnel of Edinburgh, and to experiments and observations of his own. (Sections 109, 111, 112, 113.) This argument occupies 16 pages, in which there is no mention of the production of disease, except incidentally; and no effort made to prove from Currie that cold produces disease at all. The whole object in those pages is to prove from the facts stated by Currie and others, that cold weakens the action of the heart. This being established, the author states it as the conclusion at which he had arrived, in section 138, and in 139 proceeds to state the position, for the first time, that "Cold is one of the most common, and is the most immediately active of the remote causes of fever; in many cases producing fever in a few hours, independently of the assistance of other remote causes; as in the cases mentioned above from Currie, 122, 124." These two cases are incidentally mentioned by Currie in the statement of his experiments and observations, and are above alluded to.

There is no attempt made, the author thinks, to prove that cold produces fever by weakening the action of the heart. But the propositions are considered separately and independently; 1, Cold weakens the action of the heart; 2, Cold is a remote cause of fever. On this last no proof was offered, as being undisputed. The object was, here as elsewhere, not to anticipate, but to treat each proposition on its own merits; and the reader, *from the work itself*, cannot at that stage of the investigation perceive the object of the author, or what his theory is.

The Reviewer then proceeds to give his views and those of some others on the mode of operation of cold, with which the writer conceives he has at present nothing to do, his business being to show that what he has stated as fact, is true, and what he has inferred, is correctly inferred, and no more.

The Reviewer proceeds thus; (p. 130)

"We think we have said enough to show that the author has not succeeded, to use his own language, 'in tracing the remote causes through their chain of effects to the symptoms of disease,' by assuming weakened action of the heart, from the nature of these causes, to be an indispensable precursor of all fevers, and we shall not therefore, examine into the operation of the remaining causes with reference to this subject."

The quotation in the body of this extract, though not, it is thought, in the author's own words, comes near enough for the present purpose.

As to the opinion of the reviewer, in the same extract, that he has said enough to show that the author has not succeeded in his object, the author does not pretend in that part of the work to have traced "*the connexion between the remote causes and the symptoms of disease*," but only to have proceeded so far in the accomplishment of his object as to have shown that weakened action of the heart is an effect, directly or indirectly produced, of the remote causes of fever thus far mentioned, viz. those mentioned in the second chapter of the work; the reviewer's remarks, when this opinion was expressed, having been confined to those two chapters and the preface.

The Reviewer, in the same extract, above quoted, speaks of the author's "*assuming* weakened action of the heart, *from the nature of the remote causes*, to be an indispensable precursor of all fevers." The following are the words of the author, section 691.

"It appears from the preceding pages that weakened action of the heart is the effect, directly or indirectly, of the remote causes of fever. This, therefore, is a link of the chain of causes and effects extending from the remote causes to the symptoms of fever. (18) This conclusion is confirmed by the fact long since ascertained, that all fevers are preceded by weakened action of the heart."*

From this passage it is evident, that the author, *in the first sentence*, infers *from the preceding pages*, (in which, as he had proposed in section 85, he has adduced various facts and arguments to show its truth, of the several remote causes,) that weakened action of the heart is the effect, directly or indirectly, of the remote causes of fever. *In the second*, he infers *from this conclusion*, that weakened action of the heart is a link of the chain of causes and effects extending from the remote causes to the symptoms of fever. And *in the third*, he cites in confirmation of this conclusion, *as* "*a fact long since ascertained*, that all fevers are preceded by weakened action of the heart," and quotes Boerhaave, Cullen, Darwin and Rush, in a note, in support of *this fact long since ascertained*. This is very little like *assuming* this doctrine *from the nature of the remote causes*.

The Reviewer in his observations on the chapters on the remote

* "Boerhaave's Practical Aphorisms, 563.
Cullen's First Lines, xxxiv, xlvi.
Darwin's Theory of Fever, Supplement, 1. 1. 6.
Rush's Works, vol. 3, p. 3, 4."

causes of the epidemics of hot climates, shows a disposition to speak favorably, which, the author thinks, would have been exercised more frequently but for the clashing of his facts with the reviewer's hypotheses. There are one or two remarks, however, which it is necessary to make. The reviewer says, p. 131.

"The author, *in his eagerness to exclude animal matter from any agency in the production of these epidemics*, has, we think, passed over too lightly the proofs of the power of exhalations from putrid animal matter to produce febrile diseases. Numerous instances of their agency in this respect, are not wanting in the annals of medicine, and we ought not to reject them *because they do not square with our own theoretical preconceptions*," &c.

The writer has marked in this extract two places in italics, as objectionable. The author, so far from being eager to exclude animal matter from the list of causes, was a believer *in their agency in the production of these epidemics*, even after he had concluded, as he then thought, his investigation of this subject; and it was on a reconsideration of the whole, just before publication, that his present judgment was formed. Evidence of this may be seen in an essay of his, published in the Medical Recorder for July, 1824. In that paper, after citing a number of cases on record, the object of which was to show the power of exhalations from dead animal matter, in producing fever, the writer concludes thus. "These show unquestionably, that exhalations from the putrefaction of animal matter also produce epidemic fevers." So far, therefore, was he from rejecting the theory because it did not square with his theoretical preconceptions, that *he rejected his theoretical preconceptions*, because he conceived that they were not supported by facts.

With regard to the criticism on the nature of the gas, the author agrees with the reviewer that the argument is not conclusive, and that "this branch of the inquiry is still *sub judice*." This he was sensible of very shortly after that part of the work was put to the press, in 1827, insomuch that he did not deliver that opinion to the class of 1827 or to any subsequent one.

The remarks of the Reviewer upon the subject of winter epidemics are duly appreciated. The reviewer is correct in conjecturing the meaning of the author respecting the operation of miasmata and cold in jointly producing these epidemics. If he will carefully re-peruse sections 599 to 603, he will find that they are attributed to the operation of cold upon persons *who are not yet freed* from the consequences of exposure to miasmata *during the time of their production*.

With regard to the inference to which the reviewer objects, it is sufficient to observe that, *alone*, as he has given it, the author is willing to admit that it has, perhaps, no force. But in connexion with the inferences immediately preceding, he thinks it has. We have not space to enlarge on this minor point.

The writer cannot agree with the reviewer in his remarks upon the subject of contagion. "No American physician knows enough of plague to be able to lay down, with that certainty which science demands, the laws which control its origin and propagation." If this assertion of the reviewer be correct, no country physician who has not visited places in which yellow fever prevails, knows enough of that disease, to be able to lay down with that certainty which science demands, the laws which control *its origin and propagation.* It is true, as the reviewer observes of the plague, and equally true of the yellow fever, that "on this subject, he must be content to receive his knowledge from the report of others." But having received this knowledge of the facts relating to the origin and propagation of these diseases, from others who are well qualified to report them, the physician who never saw either can form a precise and correct opinion of the circumstances in which, according to their report, these diseases always arise and prevail and terminate; and therefore can form a correct opinion of the laws which control their origin and propagation.

The reviewer proceeds to make some remarks on the second volume.

"The second volume opens with an exposition of the effects of weakened action of the heart, which is considered, as we have seen, the *immediate* and invariable consequence of the operation of all the remote causes of febrile affections, and indeed of nearly all the diseases incident to the human frame."

The reviewer is in error, in the use of the word immediate in this sentence. The author expressly says that weakened action is the effect of the remote causes *directly* or *indirectly,* (see section 691) and speaks of this effect as being gradually or suddenly produced. (See sections 1230, 1262, 1264.)

The reviewer says, (p. 135) that the author attributes the accumulation of blood in the vena cava and its great branches, "in a great measure to these veins being destitute of valves to aid in propelling the blood, and to prevent its regurgitation, whilst the external veins are guarded against such accidents, by being furnished with them throughout their extent, to accomplish these purposes."

The author attributes the congestion in the vena cava, &c. to the weakened action of the heart: he did not intend to express the idea that the want of valves in the vena cava, &c. *was the cause of the accumulation in them;* but that the want of valves in them and the presence of them in the other veins, *was the cause of the confinement of the accumulation to the vena cava, &c.* (See sections 1222, '23.)

It is impossible, in the compass necessarily assigned to these remarks, to notice all the objections of the reviewer to the reasonings of the author on the subject of the various effects of congestion on the human system. The reviewer, however, is in error in supposing that "*every physician, the least acquainted with disease at the bedside, will coincide*" *with him in his remarks.* The views given on that subject are the result of long and painful observation at the bedside of the patient. The writer, from the commencement of his practice in 1804, has been too much and too ardently devoted, for his own good, to these investigations, literally at the bedside of the patient; and the conclusions he has drawn from his observations, without pretending to claim for them entire correctness, nevertheless are so near the truth, that they have led him and an increasing number of friends and acquaintance, to practice in several diseases new to them, at least, and as successful as new. And whatever may be the doubts on this subject of the reviewer, and his friends, there are literally hundreds of persons who, without even calling on a physician for advice, are advantageously following the practice, even in that disease which the editor of the Am. Jour. of Med. Sciences has selected for his share of this correct representation of the author's practice, viz. dyspepsia.

The reviewer has occasionally expressed himself in a manner indicating a proper disposition in the office he has taken upon himself. The author is sorry to say, however, that in his remarks on the *Therapeutics* of the work under consideration, the reviewer has allowed himself a latitude that he cannot but esteem entirely unjustifiable. The reviewer in speaking of the second indication laid down in the work, viz. "to excite the weakened action of the heart," uses the following language.

"The second may be accomplished in febrile affections, by warm drinks, warm applications externally, and the exhibition of emetics, to throw the blood on the surface, and in intermittents, during the apyrexial period, by giving permanent stimulants and tonics. The author however, prefers in these latter diseases, as well as in chronic affections, the repeated use of cathartic medicines, as calomel, aloes, rhubarb, jalap, scammony, &c. given during the time of the

lowest stage of weakened action, in order to keep up and stimulate the heart's action, and to produce their evacuant effects in the after stage of excitement."

The reviewer has not in this passage given such a view of the practice, that a person unacquainted with the work, can have a correct idea of what is advised. Emetics are spoken of as the most effectual means to produce immediate increase of the action of the heart. (See sections 1551, 1555.) The mercurial cathartics are relied upon for continued use; but not in the unlimited way the quotation from the review leaves the reader to suppose. After speaking of various remedies commonly used in intermittent diseases, the author proceeds thus in section 1558.

"These remedies, however, I have made little or no use of for some years; and have derived more benefit than is usually experienced from tonics, quinine, or the solution of arsenic, from the administration of cathartics at a particular period of the remission. Cathartic medicines are necessary in all such cases; and by giving those which are stimulant, in the decline of the action of the heart towards the lowest point, the action is sustained and even raised, so that it does not fall to that point at which a chill appears; meanwhile the cathartic operation is effected at a period when it can be best borne, viz. during the increased action, and when it is most serviceable in reducing it."

In the two following sections, he proceeds to mention *expressly*, calomel, aloes, rhubarb, jalap and scammony as the cathartics intended to be used; nor has the author ever used any other with this view. The reviewer, however, by the use of the words *cathartic medicines* at large instead of "*cathartic medicines which are stimulant*," as the author has it; and by the addition of *&c.* at the end of the list of cathartic medicines given by the author, has represented him as speaking with a latitude he never intended, and does not use.

In the treatment of chronic diseases with these cathartics, there is a very particular attention paid by the author to the quantity and the quality of the discharges produced. This caution runs through the whole work, so as to constitute a remarkable feature in the treatment recommended. The cathartics employed are selected on account of producing such consistent discharges from the liver as lessen congestion, and thereby relieve the symptoms proceeding from it; (see sections 1566 to 1579) and others are rejected on account of their producing watery evacuations, which are continually objected to as weakening and injuring the patient and *increasing the disease*. (See sections 1593 to 1596 and 1602 to 1612.)

Of all this the reviewer gives his reader no notice, but leaves him under impressions respecting the views and practice of the author which

are very far from being correct. The reviewer for example, speaks of the author's "constant resort to reiterated purgation, not to be restrained even after it has *induced bloody discharges.*" The last words are in italics in the review, and certainly afford no idea of the author's views or practice. His remarks on the subject are contained in sections 1632 to 1637, and are as follows.

"1632. It is proper to mention here, that in some cases, while attempting to procure the necessary evacuations from the liver, instead of those usually observed there is a discharge of black blood in considerable quantity. This sometimes occurs early in the disease before any other discharge from the liver, sometimes in the course of the treatment; occasionally also when no medicine has been given. It always alarms the patient and his friends, and sometimes the physician. It ought seldom to alarm either if such a judgment may be formed from the following facts.

"1633. Among the whole number of my patients thus affected, I have not known more than five deaths. In one of these cases the patient was extremely ill, with the highest grade of bilious fever, and was almost entirely neglected; in fact left to his fate. I saw him not until near his death, on the 7th day, when he was covered with petechiæ. In another the bowels were not evacuated at all, though cathartic medicines were given very freely. The hemorrhage became less and less, and was never considerable. In the other three cases the hemorrhage disappeared during the use of cathartic medicines, and the patients died some time after through gross neglect of the directions given.

"1634. In all the other cases which occurred since the autumn of 1821, cathartics have been used freely, and while they were in operation the discharge ceased: and this was the case whether the discharge preceded or followed the administration of the cathartics. (1632)

"1635. In some cases the hemorrhage was manifestly and immediately beneficial. In one case the patient had been lying in a fever, with weak pulse, stupor, and pain in the abdomen for ten days. Cathartics had been given daily: when the hemorrhage appeared, the blood was discharged pure, black and fluid, five or six times in the day, in quantity equal to a moderate passage. On the next day the patient was up, and well, and walked out.

"1636. On one occasion the patient being ill, and discharging bilious matter every day, began at length to pass black, fluid blood. The physician alarmed was about stopping the hemorrhage by an opiate. Being consulted, I advised him to proceed with his plan of purging, the hemorrhage being not at all injurious. He did so, and the patient recovered, and became very fat.

"1637. On another occasion the patient having taken several cathartics without effect, at length discharged a very large quantity of blood, black and fluid. The cathartics were notwithstanding continued: the discharge of blood also continued till the third day, apparently produced by the action of the medicine; the evacuations occurring in time, and quantity, suited to the time of administering the medicine. On the third day a passage appeared of a doubtful character. It was nearly like black bile, but did not give a yellow

colour to the side of the white vessel which contained it, as bile does, however black. The subsequent passages were however more and more like bile, and in the course of the day they were decidedly so. They in the following day became less and less dark, and finally yellow; and the patient recovered."

The representation of the reviewer is the more objectionable, 1, because all the fatal cases are stated, with circumstances showing that the cathartics used, could not possibly have produced the hæmorrhage; (see section 1633 above quoted) 2, because a number of cases are stated which show that, although the hæmorrhage came on during the administration of cathartics, in the way usual in bilious fever, the cathartics could not have been the cause for two reasons.

1. The hæmorrhage from the bowels was not of that cast that could be attributed to severe purging, but a copious flow of black blood unmixed with fæces; sometimes accompanied and *even preceded by a like discharge from the mouth;* and often accompanied by petechiæ of various sizes, from that of a pea to that of a dollar, and of a purple colour, some of which being opened with a needle dark coloured blood flowed out. 2ndly, The hæmorrhage in almost every case preceded the use of cathartics, and ceased during the administration of them. These circumstances are stated in section 1634, &c. (quoted above) only in a general way, because the author was there laying down general principles of practice. But the same is stated fully and particularly in the chapter on *Hæmorrhages* in sections 2084, &c.

The reviewer proceeds with his remarks, thus;

"Little reliance is placed on promoting discharges from any other organs than the chylopoietic," &c.

The reviewer should bear in mind that the work under consideration is yet unfinished, that it embraces only those chronic diseases in which the chylopoietic functions are known to be disordered, and the remedies are therefore adapted to relieve them from the cause of almost all their disorders, viz. congestion. Hence, *in the diseases mentioned in the work*, "little reliance is placed on promoting discharges from any other organs." The reviewer will probably find other remedies for other diseases, when they come to be mentioned, used with as much perseverance, in their proper place.

The reviewer says;

"This purging is quite a passion with our author, and employed on nearly all occasions, and to answer opposite and contrary indications. To check menstruation when too profuse and to bring it on when suppressed or scanty."

As to the remark respecting the administration of cathartics *on all occasions*, the last observation of the author is sufficient notice of it.

With regard to that respecting their being "*employed to answer opposite and contrary indications*," the reviewer must have seen, if he read the work, that the author does not believe that "opposite and contrary indications" exist in the cases spoken of; and therefore that he does not employ cathartics *for the purpose of fulfilling opposite and contrary indications*. So far from this, he shows at length that the cause of excessive and of deficient menstruation is the same, and consequently that the indication of cure in the two cases is, *not opposite and contrary*, but the same.*

The reviewer continues;

"He never fears any untoward effect from the most active and stimulating materials of this class of remedies, but gives them with the same confidence in uterine hæmorrhages in the latter months of pregnancy, and in hæmorrhages of the intestinal canal, as others would employ them to relieve an impacted state of the bowels."

This passage, in connexion with the last quotation from the review, (which it immediately follows,) is calculated to represent the author as so passionately attached to purging on all occasions, as not to be deterred from the use of the most active and stimulating cathartics, in circumstances in which, according to common opinion, they would probably be destructive of life; viz. *notwithstanding the presence of uterine*

*The reasonableness of the doctrine is not here to be discussed. The writer will barely suggest to those who think it cannot be true, that congestion in the liver, produced by the remote cause of the autumnal diseases, is accompanied by increase of the secretion of the liver in some cases, and in others with a deficient secretion. Moreover, that the object in the treatment being to relieve congestion, cathartics are given when there is no secretion, as well as in many cases in which there is an increased secretion. So likewise has the writer known, in some autumnal seasons, increased uterine discharges to be common, and in others a deficiency of the same to prevail, in places noted for the prevalence of autumnal fever. Here is a striking similarity. The same remote cause prevailing, the same internal state therefore existing in those who are in a morbid condition, that internal state evincing itself by a number of its usual effects, *some* are affected with an increase, and others with a diminution of the secretion of the liver, among other symptoms; and *others* in the same situation in all respects, have the additional symptom, an increased uterine discharge, or a deficient one. With this view of the case fully explained in the treatise, the author conceived the indication was the same in the latter case as in the former; and the extraordinary and continued success of the practice, confirms him in the belief of its truth.

hæmorrhages in the latter months of pregnancy. The effect of this representation unexplained, would be to make the reader believe that the author was a madman. But the impression on his mind would be very different, perhaps, if the reviewer had done his duty in the office he has assumed. Had he performed that duty, he would not have represented cathartics as given on all occasions, without being restrained even by uterine hæmorrhages in the latter months of pregnancy, but *as given with the express view of putting an end to those hæmorrhages.* He would have examined the grounds and the arguments on which the author relied for venturing upon a practice so desperate as he seems to consider it, and he would have inquired into the result. Having done this, he would have made a faithful representation of the whole in as condensed a form as possible, that the reader might be able to judge of the propriety of the practice; and he might then have called his account of the practice recommended in the work under consideration, *a review.* As it now stands it may more properly be called *a caricature.*

The author fully sensible of the importance of the subject, and of the opposition he might expect if he published his views and his practice, gave with great particularity a statement of many cases of the treatment of uterine hæmorrhages with cathartics. He says, "From the foregoing considerations, perfectly convinced of the truth that menorrhagia is the effect of accumulation of blood in the venous cavity, I was led directly to the inference that those medicines which evacuate from that cavity would cure this affection. This was immediately tried, and the result was a speedy and perfect cure; insomuch that for some years" (now nine years) "I have been treating this disease with cathartics alone with complete success. *As this is a very important matter, a number of cases are stated.*" (Section 2135)

A number of cases are then stated in detail, in which the success was striking, immediate, and so complete, that it is believed that to this day there has not been one failure. In every case but one, mentioned in section 2149, the subject of which removed when almost well of a long continued and habitual menorrhagia, the cure is known to have been perfect.

With regard to hæmorrhages in a state of pregnancy, the author, after discussing the subject of the remote causes, &c. observes as follows. "Resting confidently on the correctness of this train of reasoning, and on the conclusion, that menorrhagia in pregnant women is

produced by the same cause that produces it in women not pregnant; (2172) and confirmed by the successful issue of the practice of evacuating from the venous cavity through the liver in the latter, I gave the same cathartics in the first case of the former that occurred, and with the same complete success." (Section 2188)

A number of cases are then stated with the same regard to giving the reader a complete account of the patient in every particular. The practice was eminently successful. Of these, as well as of the statements made of the cure of menorrhagia in women not pregnant, it is sufficient to remark, that some of them relate to some of the most respectable females in the country in which they reside; and that the cases are well known to many, as may be gathered from the extraordinary nature of the practice, and the opposition which was almost uniformly made to it when first recommended; both of which excited the attention of many to the result. Something of this is intimated in the work, (sections 2145, 46, 91,) as well as the effect of the success of the practice on those at first opposed to it. (See sections 2192, 95.)

The reviewer proceeds with his remarks thus; (p. 139)

"It would seem that in his view hardly any other medicinal effect is capable of removing disease. Has the exhibition of cinchona or the nitrate of silver cured a case of dyspepsia, it was by acting as a purgative. Has tansy or lime-water warded off the gout, or calomel and squills removed a dropsy, it was still by their purgative qualities without reference to their operation on the other emunctories of the system."

To the first sentence of this extract a sufficient answer has been twice given in the compass of the last three pages, rendered necessary, perhaps, by the repetition of the objection.

As to the remarks concerning the mode of operation of cinchona, the nitrate of silver, &c. they are calculated to make a false impression on the mind of the reader and to mislead him entirely.

The author has shown that dyspepsia is produced by congestion of the vessels of the liver, stomach, &c. by a train of reasoning which *the reviewer* says nothing about. Congestion being the cause of the disease, producing not only the derangement of secretion commonly observed, in such cases, in the liver, but in the stomach also, the fair inference is, that diminishing congestion will lessen its effects, the symptoms.

This was soon tested, and the justice of the conclusion fully supported by the result; and that the reader may have the fullest opportunity of forming a judgment for himself, a number of cases are stated

at great length and with minute particulars, from day to day, and among these were all those which were most unfavorable in their termination.

The first case, it is believed, in which the practice was adopted was the following. (See sections 1833, 1834.)

The patient had been a number of years dyspeptic, had consulted various physicians in different parts of the country, in conformity with whose advice he had made many strenuous efforts to obtain relief, without success. He had first tried tonics in vain; afterwards alterative doses of mercury and Epsom salts, with the same result; then again tonics with alteratives, and finally the Bedford water, all in vain.

By the advice of the writer, being now in an emaciated state and greatly deranged as to the secretion of both stomach and liver, "he commenced with scammony, aloes, and calomel, in equal quantity, in pills, with directions to procure about three consistent passages every day." Following this plan he soon improved, but on his second call "the discharges being rather thin, he was directed to take pills of rhubarb, aloes and calomel, instead of those first mentioned." The rhubarb was here substituted for scammony, in consequence of a condition of the evacuations continually adverted to by the writer, which is never to be allowed to continue in chronic diseases, because it reduces the patient's strength, whereas the cathartics which operate in the way proposed, bringing away consistent passages two or three times a day, may be continued to any necessary extent with a constant increase of the strength.*

*In confirmation of the above see section 1612 of the work. In the case there stated, the patient was a lady, a little above thirty years of age, of a slender frame, of sedentary habits, the mother of many children, and on all these accounts in bad health. She was advised to take the course above recommended, and long refused; but at length, when she was so reduced in strength, as to be afraid to venture into the street for fear of faintness and of falling, commenced the use of them. "She took them several months. When she commenced, she walked along slowly, with the appearance of infirmity, and upon any exertion, as on ascending a flight of steps or a little hill, her respiration was hurried, and she could scarcely proceed. After a month or more, she walked from one side of the town to the other, in a quick and lively manner, and ascended a hill without difficulty or delay." She continues in good health to this day. (See section 1612; also 1811, 1837 and 2037 in connexion; 1813 and 1841 in connexion; and section 1842.) Cases of this description, in any reasonable number, are at the service of the reviewer with names and dates. Two more relating to medical men shall here be mentioned.

In the summer of 1828, Dr. Owen of Alabama, came to Lexington for medical

The patient in question by following this plan of treatment soon recovered.

The author proceeds to give a number of cases with minute parti-

advice and called on the writer for the purpose of obtaining it. He was exceedingly weak, pale, emaciated, and dyspeptic in the extreme, so that he had been compelled to lay aside all thoughts of continuing practice and had given it up. He was advised to commence immediately with pills of rh. al. and cal. in sufficient quantity to produce two or three consistent passages every day. He followed the plan proposed and improved fast; insomuch that he went to dine out, and this indulgence was followed by a vomiting &c. and great prostration. This he at first attributed, with the writer, to the improper diet used at dinner: (among other articles were tomatoes and boiled corn.) But afterwards, having discovered that he had been taking calomel, he attributed the bad effects to that medicine. He had *objected*, at first, to the use of calomel, and the first dozen pills made for him contained none. But after the first, every parcel was made by the young gentleman in the office in the usual way, viz. with calomel, the objection not being known to him, or not being recollected. He had therefore *followed* the plan proposed, though he objected to it. His objections to continue the course, resulted in his leaving town, and at a friend's house, not far off, he treated his case in his own way. Hearing that he was likely to die, and that he was considered as my patient, I called to see him; and finding him on the bed and very low, I requested as a favor of him that, as he was not following my advice, he would let it be understood that he was not my patient; particularly as he was using some articles that in my view would not by any means benefit him. We parted in a friendly, but somewhat cool way, and I saw no more of him for perhaps two months, when I was surprised to see him ride up to the door and get down with more strength than I ever before saw him exert. Meeting him at the door, he came, he told me, to do me justice. He told me that he had persisted in the course he was following when we parted, until he was convinced he should die; and determining to make a last effort, he had fully adopted the plan originally proposed to him, had gradually regained health and strength, and was now going home. Moreover, he had, with candour and generosity by no means common in this world, mentioned every where the circumstances, determined that the truth should not be injured through him. This gentleman, the writer is informed, has become fat and healthy, and has resumed the practice.

Dr. Clarke of Missouri, called on the writer in the summer of 1829, having come to Lexington for the purpose of obtaining advice in his case. He was emaciated and so weak he could with difficulty get here. He complained of pain and disorder in the abdominal viscera. On the writer's observing that he need not ask advice how to treat that disease, he replied that he knew what would relieve it, viz. cathartics; but that the disease was so obstinate, that to cure it he was compelled to use them so often that he should be destroyed by the remedy. To this the reply was immediately made, Then you could cure the disease if you had a cathartic that could be taken without reducing your strength. Certainly, he answered. Then said the writer, I can tell you of such an one. The rh. aloes and calomel pill was then proposed to him, to be taken in the usual way. He adopted it, and in a fortnight had gained strength, and was so much improved that he returned home, confident that he could cure himself, or at least keep the disease down, so as to be comfortable.

culars from day to day, including among them all those cases which were most unfavourable in their termination. These cases occupy forty pages; and the author thinks that the numerous facts stated in them, so extraordinary in the estimation of the reviewer as to deserve the name of "*infatuation*," are deserving of other notice than a sweeping denunciation. They are not thus to be obliterated from the recollection of the writer, nor of the numerous physicians who have adopted this mode of treatment, nor of the still more numerous class of persons who have heard of it, from the contests relating to it, and have adopted it, from seeing the success attending it in their friends and neighbours, without consulting any practitioner; thereby materially affecting the practice of some physicians as the writer is well assured from the best authority, and among whom he ranks himself.

Having stated so many cases in which the mode of practice recommended, had relieved the patient in a remarkable manner, the author observes in section 1843; "These cases are sufficient to prove that dyspepsia is to be cured by medicines which produce free evacuations from the venous cavity, as above mentioned. The following cases go further, and show not only that such evacuations are effectual, in restoring health to dyspeptic persons, but also that tonics, when they are salutary, *sometimes*, and *probably often*, act in the same way."

The author then proceeds to give in the two following sections, two cases. The persons were his own friends, who suffered excessively under long continued dyspepsia. Both had made great and persevering efforts to obtain relief without success. One of them particularly, was a man of extraordinary energy, and "used tonics in the freest manner." But all in vain. These two men left that part of the country and the writer saw them no more for several years. When he met them, they were both hearty men. This excited his surprise, and he immediately inquired how they had been restored to health. One stated that he had been cured by bark, which had *purged him every day for three months;* the other that he had been cured *by bark and Epsom salts.*

From these remarkable facts the author drew *the guarded inference* stated above, that "tonics, when they are salutary, *sometimes*, and *probably often* act in the same way;" viz. by producing a loose state of the bowels.

How different the representation of the reviewer in the passage quoted. "Has the exhibition of the cinchona or the nitrate of silver cured

a case of dyspepsia, it was by acting as a purgative." This sentence would naturally lead the reader to the conclusion, that the author's passion for cathartics not only led him to give them in dyspepsia, but that he asserted without limitation, that *when bark cures dyspepsia it is by acting as a cathartic.* But the author does not make this sweeping conclusion relating to *tonics,* from the good effects of *cathartics alone;* but from observing, in cases in which tonics had long been tried and failed, and years after succeeded, that in the latter trials they had produced a continued loose state of the bowels, he concluded that they *sometimes* and *probably often* prove beneficial in this way.

As to the remark respecting the nitrate of silver, the writer will merely state the facts, and leave the reader to form his own opinion. The reviewer says, "Has the nitrate of silver cured a case of dyspepsia, it was by acting as a purgative;" thereby representing the cases in which this medicine was administered by the author, as cured by it. The following are the circumstances of the only cases in which it was administered.

The patient in the first stated, (section 1836) after long suffering the utmost extremity of the disease, was brought to the writer for assistance. No hopes were given of her recovery, but as she and her friends were desirous that an effort should be made to save her, it was made. She commenced taking pills of scammony, aloes and calomel on the third of February and continued them until the 17th. On the 17th ptyalism having come on, she took instead of calomel a quarter of a grain of nitrate of silver with the aloes and rhubarb. These medicines were continued until the 1st March, she taking every day or nearly so, pills containing from half a grain to three quarters of the nitrate with the rhubarb and aloes.

The effect of these medicines while the calomel was administered, was to produce green and black discharges daily: the effect after the nitrate was given, was to produce the same, but *much more abundantly* throughout, but particularly on the 19th, 25th, and 27th February. All this is minutely stated in the work.

The reader will observe that this is one of the two cases the author has spoken of above, as the most unfavorable. If the nitrate cured this case, as the reviewer intimates, most assuredly the author has a right to conclude that it did it by acting as a cathartic, seeing that after it was given the discharges were increased in number and in quantity. and *seeing that the nitrate of silver is an active cathartic.*

The patient in the second case commenced taking scammony, calomel, &c. on the 28th November, and continued with slight intermissions and some irregularities in the use of improper cathartics, viz. such as weaken by producing watery passages, until the 6th February, when the attendance ceased. In the course of this time the calomel having affected the mouth, on the 27th January, *two months after the treatment with cathartics had commenced*, she took nitrate of silver; and in that and the five following days used eight and a half grains of the nitrate, when she returned to the use of calomel with the cathartic. The violence of the struggle, which commenced on the 8th January, when she was very ill through imprudence mentioned in the case, was over by the 12th, and from that day she improved regularly; viz. fifteen days before the nitrate was administered. And the reader will observe that all this is minutely stated in the detail of the case from day to day.

What renders it more remarkable that the reviewer should attribute the cure in this case to the nitrate alone, in contradistinction to cathartics, is, that the progress of the disease and the way in which it was treated by various physicians, is noticed in a very particular manner in section 2037, and the whole shows in a remarkable manner the efficacy of cathartics throughout the whole period, and the inefficacy of every other mode of treatment that was tried.

The reviewer is not more correct in his remark respecting the author's views of the operation of tansy and lime-water in gout.

The author's observation of the causes and the nature of gout, led him to the conclusion, that the immediate cause of the symptoms is congestion, and consequently that evacuations from the liver by the bowels, should relieve the patient. His own experience in years previous to the time of his investigation was in full accordance with this conclusion; and the experience of many deservedly esteemed medical writers was equally so.

Dr. Cheyne says he knew a quack to administer sixty grains of rhubarb disguised with cochineal, every morning for six weeks, to a gouty person, who had no symptom of the disease for four years afterwards.

Dr. Rush says he knew two persons who had been accustomed to two fits of the gout a year, who had been preserved from them several years by taking sulphur to obviate costiveness.

Dr. Clark states that he had a patient who had had the gout ten

years, for two months of the twelve in some of the latter years, who drank every day a quart of lime water, which purged him two or three times every day; and latterly when it seemed to lose its laxative quality, he directed him to take four ounces more, after which it purged him as before. He had had no fit nor any other ailment at the end of ten years, when the statement was made.

He states another case, in which a gentleman, who had the gout so severely as to be confined every winter two or three months, and sometimes a month in summer, took near a pint of strong infusion of tansy every night, and had two easy passages every day. He had no return of the gout in seven years, but once slightly when he strained his ankle.

He mentions another case in which the same remedy had a good effect, and the patient had two easy passages every day.

He mentions another in which a patient was entirely relieved by taking half an ounce of the elixir sacrum, every second night. This dose contains the substance of seven and a half grains of rhubarb and four and a half of aloes.

From these statements, all made in the work, the reader may judge whether the author was right or wrong in his opinion.

The author was so far from attributing the good effects of calomel and squills in dropsy to their cathartic operation *without reference to their operation on the other emunctories*, that he had used that remedy for years *with reference to its operation on the kidneys alone*, notwithstanding that he continually observed it to purge the patient. And when his investigations led him to believe that it was by its cathartic effect, so far from having no reference to its operation on the kidneys, and throwing it aside for a mercurial cathartic, he gave the calomel and squills in the first case that occurred, and watched the effect on the patient. "They operated on the bowels; the discharges were precisely such as are usually produced by pills of jalap, aloes and calomel. After a day or two, the latter were used and continued until the disease was entirely removed; which was in a short time. The patient was young, and the disease has never returned. This was above five years ago. This was considered, as it is, a strong confirmation of the truth of the doctrine; it being evident that the medicine used as a diuretic had produced its good effects by purging the patient. In consequence of this it was determined to make a full trial of the pills of jalap, aloes and calomel in the treatment of anasarca, ascites and hydrothorax. The

success of the practice was such in the first cases, as to hold out a strong inducement to continue it." (See section 2407–8.)

A number of cases are then stated, which support this practice in the strongest manner, and show beyond all doubt, that whatever other medicines may do, those advised are very useful in the treatment of dropsy.

The reviewer continues to make remarks upon the character of the work, which he does not support by reference to particular passages he has in view in making them; and the author therefore, can only say, in general terms, that the doctrines advanced in the work are *not* rested upon his own *ipse dixit*, nor upon the authority of great names. The author cannot spend his time in searching through the whole two volumes, to make sure that there is not some slight reference to authority which might be construed unfavorably to the following declaration, but *he fully believes that there is not a solitary reference to authority for any doctrine.* There is undoubtedly reference to great names *for facts* upon which the doctrine is built; but the author is yet to learn that authors of great and deserved celebrity are *not to be quoted for what they have seen or heard.*

As to the remark about novelty, ("*Well may the author observe that the treatment is, as far as he knows, new!*") the author will only observe, that he recollects no part of the work in which claims to originality are obtruded on the reader. There is a note to section 1537, in which he states the simple fact about a matter of small moment, not with a view to such a claim but as a reason for not quoting as the author of it, a writer who has also advanced the doctrine, when he did not derive it from him. The remark of the reviewer is made in connexion with his observations on the treatment of dyspepsia; and on turning to that chapter the following passage was observed at the close of the first case. "The unquestionable importance of the case, and the successful termination, will, I am persuaded, fully justify the space occupied by it. I shall state others as fully, because one case is not to settle the question of the propriety of *a mode of treatment novel as far as my reading extends.*" Surely nothing can be more inoffensive than this manner of mentioning a new practice. There is no claim of credit for the novelty of the treatment, but its novelty and extraordinary nature, admitted by the reviewer, is simply mentioned as the reason which induced the writer to put the reader in possession of abundant materials to guide him in his judgment of its propriety.

"This similarity (says the reviewer) is not, we suspect accidental. At any rate we have no where the least intimation that the author is acquainted with the labours of Parry, Armstrong, and Abercrombie of Britain, or with those of Louis, Andral, and Laennec of France, with a host of others, who have contributed to base medical science on principles unknown to former times."

Does not the reviewer perceive that this very fact, that there is in the work no such intimation, is evidence that the author was not then acquainted with those authors? This would surely be a rational conclusion for a plain, sincere man to make. The reviewer professes to know nothing of the author. Knowing nothing of him but from his work, and finding nothing in the work to indicate that he was acquainted with the authors he mentions, what are the grounds of *his suspicion?* Is such conduct *universal* in authors? Certainly not. From what internal feeling, then, springs this *suspicion?*

The preface to the work seems, for the most part, to have been lost to the reviewer. He might have gathered from it, that the writer was for a long time, (until the beginning of 1822,) engaged in a laborious country practice, in which of course he had little time for reading: and during which, he in fact had few books to read. "The long and solitary rides he was frequently called upon to take, afforded opportunities for undisturbed reflection," (p. vi) but little for any thing else: and the result of his investigations was, that "The doctrines advanced have been familiar to his friends *since the spring of* 1822." (p. viii) The case of hæmorrhage stated in section 2085, occurred in February 1822; the case of menorrhagia (2136) in May of the same year. In that year and the following, many such, with cases of dyspepsia, hysteria, gout, &c. were treated in the same way. On the other hand, of the works mentioned by the reviewer, the writer had never then heard, certainly never seen one except Armstrong.

The French works on this subject which the reviewer mentions he has never yet seen, and does not believe have been published in this country; Parry's work, it is believed, has never been republished in this country, and the author never saw it until he met with it in the Lexington library since the publication of the work in question, and more than six years after the date of some of his cases; Abercrombie's work was published since the treatise of the writer: the preface to that work is dated in 1828, and the book was republished in this country in 1830; *the treatise* was printed in Winchester, Virginia, before the writer was called to Lexington, in 1827, (except about fifty pages,) as stated in the advertisement prefixed to the pre-

face. The reviewer's suspicion with regard to these therefore is unquestionably groundless. The work of Armstrong alone remains to be mentioned.

This work was republished by J. Webster in 1821, who sent the author a copy in the following winter, with three other books mentioned in the bill, of which one was "Surgical Essays, by Cooper and Travers," to which is prefixed a dedication to Dr. Davidge dated Nov. 1, 1821. *The bill is dated Dec.* 4, 1821: one of the author's cases is dated Feb. 1822, and in May of that year the practice deduced from his investigations, was in full operation in Winchester, Virginia. (See sections 2085, 2136, &c.) At what time in the winter of 1821, the work of Armstrong reached the writer cannot be ascertained. But whenever it was, he was in no condition to sit down deliberately and commit a robbery. In December and January his family were in great distress from sickness of himself and others. February, March and April were spent by him, as is partly mentioned in *the preface,* (p. viii,) in travelling to seek a place of residence, in removing his family to Winchester, (preface, p. viii) and on a bed of sickness in it, from which he was not expected to rise. In this time of harrassing care and laborious exertion, resulting in severe illness, it was impossible *to read carefully* such a volume; much less to digest it, with its views directly opposed, in capital points, to those of the author, into a system leading to a practice so novel, and to give it such "*extensive application*" as the reviewer allows it. (p. 126) In truth the practice alluded to had been fully adopted before a page of that work had been read; a circumstance not remarkable when it is considered that the subjects treated in it are fevers, and that the attention of the writer was then particularly turned to the chronic diseases.

The author has reserved for this place a passage of the review, because the editor of the Journal has appended to it a note, with a reference to an article of his own on the subject, which, alike in spirit, may be noticed together. The reviewer, in remarking upon the treatment of individual diseases, after mentioning blood-letting, says; (p. 139)

"But in general the great reliance is on purgation, repeated again and again *without looking to any ill consequences that such a persevering course may induce.* The infatuation with which the use of pills of aloes, jalap and calomel, is persisted in day after day, till the patient has taken in a case of dyspepsia, not grains but ounces, and we might almost say, pounds, is really incredible and consternating."

The reader would scarcely believe from this and similar passages of the review, that the author *had continually in view* limitting the purging to two or three daily discharges of such a character (consistent and not watery) as *to prevent weakening the patient*, and that this is adverted to in *many* of the cases and *often* urged upon the reader as indispensable.

The writer cannot conceive the propriety of talking about the quantity of medicine a man takes in a long continued chronic case. The reviewer would not object, perhaps, to giving bitters, steel and bark, for months, and years with Dr. Whytt*. He would probably make no more objection than the author, to "the almost unremitting continuance of purging in hydrocephalus;" or to the use of cathartics day after day, without interruption, for several months successively, in epilepsy; or to copious purging in the confirmed marasmus of children, notwithstanding the great debility present in those cases; all recommended by Dr. Chapman;† or to the long continued purging with jalap and crem. tart. in the disease of the hip joint, recommended by the venerable Physick. The quantity does not startle him in these cases, although he may here also easily reckon it by ounces, and almost, perhaps altogether, by pounds. And why? Because he is persuaded that it is the proper course in the respective diseases. So here. The question is, What is the proper course? The author has reason to believe that *two or three daily discharges* from the liver through the bowels, will, often in a few weeks, most effectually relieve the diseases treated of in his second volume. He moreover proposes, as the best prescription which his observation and experiments have led him to adopt, *as the mildest and least exhausting*, the pill of aloes, rhubarb and calomel. In most cases of dyspepsia, two pills, containing ten grains of this compound, produce the desired effect; sometimes one: and seldom are four required. The few exceptions, two or three, certainly not half a dozen in nine years, were cases which ran into an epidemic season in which extreme difficulty of moving the bowels was a feature of the prevailing epidemic; in which case they, as has often been observed of diseases at such times, assumed this feature; and, like the other cases of disease then occurring, required larger doses to produce the usual effect; and the only

*Whytt on Nervous Diseases, p. 337.

† See for the former, Phil. Jour. Med. Sciences for Feb. 1827: and for the two latter see Mat. Med. vol. i. pp. 169. 178.

choice left the physician and the patient, was to give up the hope of relief, or seek it in an increased quantity of the medicine. Such was the case which the reviewer alludes to, and which the editor of the Am. Jour. of Med. Sciences has selected for his animadversion, and to represent as a specimen of the author's practice; with what propriety the following remarks will show.

Remarks on an Editorial article in the Am. Jour. of Med. Sciences for May 1831, *relating to the treatment of dyspepsia.* By JOHN E. COOKE, M. D.—The editor of the Journal abovementioned, in a note at the bottom of one of the pages of the review of C. D. (p. 139) says, "In our periscope, under the head of American intelligence, we have inserted two cases of dyspepsia related by our author. *They afford a specimen of his purgative practice in that disease.*" In the place referred to, the editor observes, "In the review of the Pathology and Therapeutics of Dr. Cooke, which will be found in another department of this number, the extent to which the author of that work administers purgatives in the treatment of dyspepsia, is spoken of, but the quantity said to be taken in some cases, is so extraordinary and unparallelled in the records of medicine, that it would be impossible for the reader to form any notion of the practice, without an example: we therefore extract two cases from the second volume of the work, and *that the author may be fairly represented,* we shall quote them *at full* in his own words."

The editor then states two cases, which certainly do not afford the reader a specimen of the ordinary practice at least, of the author in dyspepsia; neither is he thereby fairly represented.

The reader will observe that the author mentions two or three daily loose passages, as the object to be aimed at, and thin and watery and abundant passages as most sedulously to be avoided, *in many places* throughout the work; not in laying down general principles only, but in many of the cases stated occasional changes for the worse are pointed out, and attributed to the patient's taking cathartics not recommended, or more than he was directed of those which were, whereby he was purged too freely and injured.

The reader will further observe that the editor says not a word of all this; but gives *as a specimen of the author's practice,* two cases

which are striking exceptions to the general rule as to the quantity of medicine; and the reader not having the rule of practice laid down by the author, but only the exceptions, is naturally led to consider the exceptions as a fair specimen of the practice under the rule; and what renders this more exceptionable in the editor, is the fact that the author speaks expressly of these cases in the following terms.

"It is proper to observe that *these extraordinary doses have not been necessary in any other cases than those recited.* In general four pills of rh. al. and cal. or of jal. al. and cal. act sufficiently. To these common doses no one would object, if good arise from the use of them. If objection be made to *the extraordinary doses*, the following is the point to be decided. When ordinary doses fail to produce an effect, which when produced will certainly be followed by improved health, shall we give extraordinary doses, or allow the patient to continue to suffer? The decision rests on the same ground in this case as in autumnal fever. Evacuating bilious matter is necessary. If one, two or three doses fail, we do not stop, but persevere until the operation is effected, without limit as to quantity. If we succeed in purging the patient, he recovers; if not, he dies; and numbers are now saved by perseverance who but a few years ago would have been left to die." (Section 1840)

The author, moreover, not only speaks of them in these terms as extraordinary cases, but in relating the cases referred to in 1840, (viz. the two quoted by the editor, in sections 1837, 1838, and another in 1839) mentions the reason of this extraordinary state of the bowels in them. In relating the second case stated by the editor, the author observes "that the necessity for large doses was not confined to him; but *that in this season it was a general observation among physicians, that uncommonly large doses were requisite to produce the usual effect on the bowels.*" (Section 1838, p. 247.) This was the 10th of August: he took in the commencement, in April, but two pills daily. The first case stated by the editor as well as one of the same character not noticed by him, occurred in 1824; the former in the close of the autumnal season, the latter earlier. In the commencement this patient took moderate doses for several months with great advantage; but like the subject of the last case mentioned by the editor, "about the first of August after getting wet he became worse, and the season being sickly the disease assumed something of the character of the prevailing epidemic, and required even larger doses than in the former case." (Section 1839) The year 1823 was a remarkably sickly year in Winchester and the neighbouring country, and in 1824. 25 and 26. there were considerable epidemics.

and particularly in the last. In all these, great difficulty of moving the bowels was experienced.

It is evident from these extracts that the editor has not given a fair representation of the author's practice in stating the two cases abovementioned. So far from exhibiting a fair specimen of the practice, it is evident that the author felt the difficulty of his situation *in being compelled by the urgency of the case and of the patient to go beyond all his previous experience in order to procure relief.* Thus in the last case stated by the reviewer, *the patient was so relieved when the passages were obtained and so oppressed when they were not,* "*that he begged for something to bring off the black matter, which he was always glad to see.*" And in the case not stated by the editor the same was observed. "His discharges were dark; his bowels were very difficult to move; if a discharge from them was not obtained, he suffered very much from all the symptoms, at the same time his complexion becoming darker; *he was therefore continually anxious and urgent that this should be effected,* and if the medicine failed, sent off without hesitation a long distance to give information and get more medicine. *Thus situated it was absolutely necessary to effect a passage, even for present relief.*" (See section 1838.) This patient was himself a physician, and stated to the writer that he had, before applying to him, consulted a physician, whom the editor would admit to be one of the first in Philadelphia, and had perseveringly followed the advice he received for eleven months, without any material benefit.

These observations shall be closed with a few remarks on the cases stated by the editor.

In the first, it is to be remarked that almost from the commencement, the patient took, in aid of the medicines recommended, large doses of magnesia, which is specially objected to in more than one place; (see sections 1566, &c. 1609 to 1612) and which was frequently objected to by the writer in the progress of this case. Notwithstanding this irregularity, however, although the treatment commenced late in November, by the 9th of December she was much relieved, and the attendance ceased for the time. (See the case stated by the editor.)

Having neglected the advice given her, to continue to procure two passages daily, she had on the 14th of December a return of the symptoms with great violence. She immediately commenced taking merc. cath. pills, without magnesia. "These medicines operated pretty well, and she improved daily in every respect," until the 19th; when the attendance again ceased for a time.

From the 19th to the 29th she took medicine occasionally only "The acid gaining head, she took on the 30th magnesia with a little soda, and twelve grains of aloes;" and until the 3rd of January took magnesia and senna. "The discharges from these medicines were all light coloured and thin, and she was getting worse; so as again to request my attendance."

January 3rd to 8th, took merc. cath. and occasionally magnesia, almost in every instance of her own prescribing, instead of repeating the merc. cath. when the first dose failed to operate. The consequence was, "that the discharges continued thin and light coloured, and she was rapidly getting worse." This had been predicted, and was urged strongly, and "she was at length convinced, to use her own words, 'that nothing but thorough work would do,' and determined to give up the magnesia." "Jan. 8. She was by this time very low." From the 8th to the 11th, inclusive, she took merc. cathartics alone, resolutely withstanding the inclination to take magnesia, which had by its speedy operation always afforded her temporary relief, but which had no lasting good effect, because it did not evacuate from the liver, and at the same time weakened her continually. (See sections 1612, and the 3rd case in the note at the bottom of p. 310 of this Journal.) This abstinence was rewarded, and as soon as the merc. cath. produced black discharges she obtained relief, and these being continued, she improved every day. After the 12th there was no acid. Feb. 6. The dose was reduced to three pills. "The discharges became natural in consistence about the 21st January. The appetite had been for some time very good; no acid; no pain; no waterbrash; strength returning; colour good and healthy. Continued to take pills of rhubarb and aloes occasionally, after my attendance ceased." She continued well in September 1829, (near five years afterwards) by the occasional use of the pills.

This abstract shows in a striking manner the effect of cathartics in dyspepsia, and *the difference between cathartics of different kinds;* and places in a strong light the fact, that stating cases *without the rule that governed the practitioner in the conduct of them,* is not the way to represent him fairly, or to give a true specimen of his practice.

In the statement of the second case, the editor does not "quote it at full in the author's own words," as he proposes to do.

The editor remarks that "the author gives a very brief detail of the progress of the disease from the attack, up to the summer of 1825,

which as it does not bear immediately upon the treatment subsequently pursued, we shall omit."

The author gave all the facts he could extract from the patient in repeated conversations, taking notes, reading them to him and hearing his corrections; and gave the whole, though he was afraid they would be considered long, (occupying four pages, 1812, 1838) because he considered the most of it as "*bearing immediately upon the treatment subsequently pursued*," and showing in a remarkable manner that in the course of ten years, in the hands of various physicians, every remedy that produced a loose state of the bowels, was of service to him, and every one that did not, was of little or none. Among others is the statement that "a box of pills and a mixture of rhubarb and salt of tartar, *which together produced three or four dark green passages a day*, had a very good effect, and he thinks if he had continued them he should have been cured. But he got so much better that he went to work, laboured very hard in preaching, and became as ill as ever."

The editor regrets that, as the dyspepsia followed an attack of fever, the treatment of the fever is not mentioned. For, he says, "If the purgative plan recommended by the author, was employed, there would be much ground to suspect that the disease was induced by the use of the remedies; as one of the most common cases for which we are here consulted, *by patients from the South and West*, are the various forms of chronic irritation and disorganization of the abdominal organs, induced by the remedies employed for the cure of the fevers of those sections of our country."

What can be the object of a writer, who with the book in his hands, casts such insinuations in direct opposition to the facts stated which he has thought proper to omit?

In the case stated *in the treatise*, it is said that this disease commenced after a fever in 1807. At that time the author lived in the East, the patient lived in the East, and was treated by Eastern physicians, taught in Philadelphia, if taught at all. This the editor with the book before him must have known. (See preface and section 1812.) Moreover, this is true of almost every case stated. Physicians taught, directly or indirectly, in Philadelphia, managed the fever in every case, and treated the dyspepsia that followed.

The writer has only room to remark respecting the close of this case, that it is not stated in such a manner as to give a correct idea of it. The patient by the middle of September had become able to eat freely

of mush and milk. By the use of toddy from early in the morning he lost his appetite, and on giving up this indulgence it became as good as it was before. He continued improving under *the occasional use* of the pills; and in November the disease returned, the patient after attendance had ceased, having returned to his indulgence in toddy, (see pp. 251–253 of the treatise, vol. 2) and was given up in despair.

As to the insinuation respecting the practice of the physicians of the South and West, it is arrogant and presumptuous, and altogether absurd. 1. In presuming that the practice recommended by an individual in a publication in 1828, should have had the extensive influence the editor seems to attribute to it. He ought to know that there are very many distinguished practitioners throughout the South and West, whose practice was not to be formed by so recent a publication. Has the editor's practice in dyspepsia greatly increased since the publication of the work under consideration? 2. In presuming, at the distance of 600 miles, to say how the physicians of the South and West ought to practise; and that by the remedies used by them for the cure of fevers, various forms of chronic irritation and disorganization of the abdominal viscera, are induced.

The editor in the last three lines of his notice of the treatise observes;

"It must be remarked, that, it is acknowledged by Dr. Cooke, that these extraordinary doses have not been *necessary*, except in the cases which he cites, and two of which we have copied."

It is rather late at the close of a notice of six close pages, intended to give edge to a review preceding it, to make this solitary remark. The editor has deliberately, at full length, given the character of *extraordinary* to the practice of the author, in connexion with the charge of *infatuation*, (see p. 317) and endeavoured to support it by stating two cases as a specimen of that practice; and even when he admits in three lines at the close that they are stated as extraordinary cases, he says nothing about the ordinary practice; and neither in the review, nor in the editorial article is there any precise statement of what the author proposes to effect and what he considers it necessary to avoid.

THE

TRANSYLVANIA

JOURNAL OF MEDICINE

AND THE ASSOCIATE SCIENCES.

VOL. IV. NO. III.——WHOLE NO. XV.

FOR JULY, AUGUST AND SEPTEMBER, 1831.

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ORIGINAL COMMUNICATIONS.

ARTICLE I.—*Strictures on the new French Doctrines.* By S. HENRY DICKSON, M. D. *Professor of the Institutes and Practice of Medicine, in the Medical College of South Carolina.*

(Read before the Medical Society of South Carolina, in July, 1830.)

I APPEAR before the Society on the present occasion, in compliance with an arrangement entered into so long since, that it had been forgotten, until I was, within a very short period, suddenly reminded of it. Little opportunity has therefore been afforded me for the selection of a topic which might afford entertainment or instruction in its discussion, and I have thought that I could not do better than offer you a brief record of a few of the latest and most important discoveries and improvements in our divine art. Such a register, brought up from time to time, might, it seems to me, be specially useful. The visions of optimism, if they *are* not true, *ought* to be so,

as they are peculiarly flattering and delightful; nor can they ever be realized, unless the sanguine hopes of enthusiasm can be encouraged, and made to excite the young and ardent to zealous and persevering action.

There never was a time when there existed a prospect so exhilarating of realizing these dreams. Bacons and Newtons have abounded of late in our profession—nay a very Mohammed has arisen, who, aided by a few devoted converts, has propagated the true faith with a rapidity and success beyond all former example. A royal road has been opened to Therapeutics, through the broad realms of Physiology and Pathology; the key of the innermost apartments of medical knowledge has been found, and all are admitted through the open door. There used to be some obscurity in these matters—"mais, nous avons change tout cela"—and all that remains is to gaze steadily without being dazzled at the flood of light which has been suddenly poured in upon our astonished senses. And this light has shone too from a most unexpected quarter. We were accustomed to see Frenchmen foremost in war, though at last always beaten—building the best ships, to be captured by their dull insular enemies—inventing the most ingenious machinery, to be set in operation at Birmingham and Manchester—and sending forth to the red fields of carnage Larreys and Pereys and flying ambulances, as well as Dessaixs, Dumouriers and Buonapartes. We were not indeed prepared to expect them to lead in medicine as well as in surgery and mathematics; yet truly something might be anticipated from their familiarity with the severer cases; and their experience in such must be very extensive, if, as Hawkins affirms, they have been losing *one* patient in *six*; while their neighbours buried not more than *one* in *sixteen*. Strange as it may appear, however, it is from France that we have received the precious shower which has descended upon our barren and thirsty profession.

I proceed to mention, in detail, some of the discoveries to which I have alluded.

And, in the first place, it is ascertained that disease is a unit,

like the French Republic "one and indivisible," and consisting only in definite degrees of "intensity of action." It has indeed been said that Brown and Rush and Parry knew this well enough; that Parry taught that all diseases depended on vascular force of determination and impulse; that Rush argued that "a diversity of symptoms no more disproved the unity of disease, than the diversity of colours proved a plurality of suns;" and that Brown constructed a regular thermometer of life and death, health and sickness, upon this very principle. It is evident, notwithstanding, that the discovery was bona fide reserved for our French Professor, as Fulton may be said to have invented the steam boat, although his was not the first that was actually made. Neither Brown nor Rush nor Parry knew any thing about "the Tissues," so all important in Therapeutics; Rush used purgatives of the most heterodox character; Parry fixed his attention too much upon the large vessels; and Brown was misled by his notions of debility. It is well observed by one of the first of modern writers, "that many things are known before they are discovered," and such was perhaps the fact in the instance before us.

If it be farther alleged that the doctrine, by whomsoever invented or advocated, is not true; that disease is not a unit; that maladies differ among themselves, not only in degree and in location, but also in kind and nature specifically, it will be sufficient to reply that this is "Ontology" and "Essentialism" and that the French School have discarded it forever and totally.

2nd. Next it is ascertained that all agents which affect the excitability are irritants—irritation being conveniently defined to consist in "increased intensity of action." It may be said that Brown maintained this point also; but this is denied, for he calls such agents *stimulants*, never using the word *irritant* or irritation; and it is well known that words are things. Besides, if he did touch on this matter, he still left it a terra incognita, and before the Frenchman no one had landed on it and taken formal possession.

3rd. Disease consisting always in mere irritation, and irritation being of necessity a local phenomenon, no disease can

be properly said to be in any sense of the term constitutional: in this respect there is no accurate pathological difference between a Gastro-Enterite, (a local affection some lustrums ago called "Fever,") and a wart or a corn.

We do not mean to be understood that one local irritation may not excite more, and others fewer connected and successive local irritations, but that these are incidental altogether, and by no means "essential." In regard to this doctrine we are sure that we may claim the credit of originality, as we differ from Brown and from every body else.

4th. The subject of fever has been briefly touched. The very word is exploded as an unscientific abomination, never henceforth to be mentioned to "ears polite." That condition of the body so called of yore, is now made out to be nothing more nor less than an irritation of the mucous membrane of the stomach and intestines, and is in all cases absolutely identical in nature, seat, and proximate cause, whether it succeed a gun-shot wound of the knee joint, an exposure in a summer bivouac in a swamp, a dose of corrosive sublimate, or an insertion of variolous matter under the cuticle. This is shown in ten thousand post mortem investigations in which this membrane has invariably shewn traces of irritation; it is sometimes *pale* as Hamlet's Ghost—sometimes *red*—sometimes *just between* —sometimes *softened*—sometimes *hardened*—sometimes *thickened*, by hypertrophy—sometimes *thinned*, by absorption and ulceration—sometimes *tough*—sometimes *brittle*, yet always, without exception, IRRITATED.

The opportunities for these enquiries have been unequalled in number and extent: for, as has been already mentioned, sixteen pr. cent die in their public hospitals; all who die are subjected to close inspection and no body is inspected without disclosing a regular Gastro-Enterite, however little it may have been thought of before.

5th. It has been laid down that all agents which affect the excitability, irritate—in other words "increase the intensity of vital action." It is therefore evident that to the mucous membrane of the Gastro-Enterite surface no application whatever

must be made, with remedial views. There is however an exception which must not be forgotten. It is ascertained that mucilage, and mucilage alone, has the wonderful power of lowering the intensity of vital action—of diminishing irritation. This it probably effects according to the yet imperfectly developed laws of "Endosmose and Exosmose;" now as the whole class of purgatives are violent irritants and have nothing to do with either Endosmose or Exosmose, they are poisonous to a patient labouring under Gastro-Enterite—no scientific man will hereafter dare to cure such a patient with purgatives. If he has hitherto done any thing of this kind, from unpardonable ignorance and mental blindness, he must now repine in sackcloth and ashes, and having made due confession of his errors past, "go and sin no more." Nay farther; as it has been pretty plausibly calculated that two thirds of mankind have been in the habit of dying of Gastro-Enterites, under the odious name of fever, and as the greater portion of these, in all probability, yielded to the force of the poisonous and incendiary medicaments employed, rather than to any inherent necessity in the case, it is altogether reasonable to believe that, by the mere abandonment of this reprehensible practice, even if nothing better be substituted, a saving will be made of half the annual number of deaths throughout the world, and it is conceived that the worthy Professor of Val de Grace is as much, if not more, entitled to the veneration and substantial gratitude of civilized communities, as Dr. Jenner of vaccine fame, or Mrs. Stephens of lithontriptic memory. That the promised results have not been made evident is no doubt owing to the fact, that the constitutions of the present unfortunate generation of human beings are already ruined with mercury and other cathartics.

6th. The most important however of all the alleged discoveries is probably the improvement in medical logic. Formerly Physicians were content to reason *a posteriori* in all practical or therapeutical affairs. They were content to observe carefully facts as they occurred, and if the succession were repeated, they held that one was the cause and the other the

effect, following in this rule of "post hoc propter hoc" the example of the dangerous sceptic Hume, and advocating the method of induction proposed by the corrupt Judge Bacon, "the wisest, greatest, meanest of mankind." They went so far as to contend resolutely that if men got well of Gastro-Enterites after being purged and mercurialized, it was because of the mercury and cathartics that they recovered, and arguing "onward from things known to things unknown" they perversely concluded that there existed in mercury and cathartics some virtue by which directly or indirectly they were adapted to the cure of Gastro-Enterites. No error can be more obstinate or dangerous. The reasonings now commence at the other end of the chain and proceed in true a priori form. The French doctrine is called the Physiological doctrine, because it is based upon our knowledge of intimate Physiological function, as connected with and dependant on intimate Anatomical structure; and as all these secret recesses have been laid open by the exertions of the French School, and every natural and healthy action is *perfectly well understood*, it is plainly our best mode to follow this clue which will not fail to guide us out of the labyrinth. We know the exact tint of colour of a healthy mucous membrane in a living subject. We *know exactly* the changes which may take place in death, that tissue being undiseased, (if such a circumstance be within the range of possibility,) of course we know exactly upon inspection post mortem the nature and degree of disease which may have affected it. So also of arachnites and all other irritations of all the other membranes and tissues of the body. As the pathology is thus inferred from the physiology of the case, we go on to deduce the therapeia from the pathology. We see in every dead body marks of more or less irritation—proofs that the vital actions were carried on in some of the tissues or organs, with too much vigour and intensity. From this we learn the necessity of employing in all cases the promptest and most efficient means of reducing the force of the vital actions—*all cases*, therefore, must require *abstraction* and *mucilage*—divina remedia—magna Dei dona!

7th, and last. The correct value is set upon the disclosures of Pathological anatomy. It is true that Physicians have always considered the inspection of dead bodies as a matter of some consequence, and that morbid preparations, of which the history was well known, were regarded as important indications of the force and direction and effects of specified maladies. It was even allowed that in some cases the seat of a disease might thus be made known, and suspicions that had been entertained confirmed or removed. But until lately it was never dreamt that we could read in a dead body the whole history of the rise and progress of the fatal distemper—that we could shew the initiatory impression, the earliest footstep of the destroyer—follow him through the sympathetic and revulsive communications of irritation and disorganization, and distinctly separate, as we proceed, effect from cause in regular and definite succession. Yet all this can now be done, and much more; and the North American Savage is not more acute in tracking his friends and enemies by a broken twig, a dry leaf displaced, an ant's nest demolished, than a well trained Pathologist of the French School in pursuing his devious way through the mazes of an autopsy. Nay the time is at hand, if it be not already arrived, when all the data required for the solution of any pathological problem shall be the corpse, a dissecting knife and a lens—when we shall be able to tell to a certainty whether the subject examined, died of calomel, castor oil, ipecac, or magnesia.

In conclusion it is perhaps proper that I should offer to the Society an apology for having thus treated in irony, questions of such grave and serious import. My apology is this. It does not consist with the defects of humanity to hear with unmoved temper the unsparing ridicule and vehement denunciations heaped by the followers of the New Doctrines upon their unconverted brethren. Under the sneering appellations of "Autologists," "Essentialists," and "Incendiaries," we of the Old School are alternately taunted and reviled, until forbearance has ceased to be a virtue and the gauntlet must be taken up. We reserve to ourselves the privilege however of making

our reply in the same spirit in which we are assailed. Scorn shall be met with "retorted scorn," argument when it comes, with argument, fact with opposite fact. We cannot expect however to silence antagonists so loud that they can hear nothing but their own voices—antagonists who would blot out the experience of the long and revered list of our predecessors, and, attributing all the successes of former practice to accident, set no value on testimony, and throw away all the economy of the Materia Medica.

But we will shew a reason for the faith that is in us, and make good our position before the thinking portion of the community. I have said we will answer argument *when it comes*. I spoke advisedly. We have propositions enough—statements—expositions—but these are not satisfactory. We are not content with histories of cases, of almost interminable duration, on both sides of the Atlantic. We will readily acknowledge that patients may get well under the French treatment, which we believe is not capable of much positive harm, except in its being substituted for better. Our opponents go far beyond us in this matter, for they have some of them had the generosity to concede that a portion of our cases may get well even against the disadvantage of a positively irritating, injurious, poisonous, and incendiary interference. (I borrow epithets here from authority.) There are many difficulties in the way of our conversion. The Koran of the sect remains yet untranslated and therefore to the unlearned is a sealed book—though to this objection it may be replied, as it was of old by the followers of the Eastern Prophet, that there is the more faith, and therefore the more merit in receiving it the less it is understood. We look in vain for some faithful commentator who shall give us an accurate transcript of each dogma as revealed. One translator (I believe the only one) while recording the favorite practical *prohibition of purgatives*, declares in a note that "cathartics form an essential part of the treatment of the fevers of North America." (Translation of Begin by Jessier p. 23.) Dewees in his New Practice of Medicine, while in his 10th page he sends in his adhesion to the New Pa-

thology, and in his 11th offers the most adulatory incense to "Broussais and his friends," lays it down positively in his 8th that "in fevers of almost every description purging is not only useful but in many is indispensable." Nay we are surrounded by many physicians who, unwilling to seem to be left behind in the march of professional improvement, eagerly range themselves under the gallic banner and avow themselves ready "jurare in verba magistri," yet so far as our utmost ingenuity can discover, agree with him in no one point of practice. Thus Dewees cannot dispense with purgatives, although warned of their dangerous properties, and although he has seen "three tablespoonfuls of the animal solution called chicken water (p. 19) create so much fever that seven bleedings were required to subdue it." The numbers being so carefully specified as to enable us to calculate with arithmetical accuracy that if the patient had taken nine spoonfuls, twenty-one venesections would have been demanded. Indeed among the numerous physiological physicians whom I rank in my list of personal friends, there are but three unmodified, and uncompromising. These shrink from the approach to an apothecary's shop; suffer an horripilation at the bare mention of calomel and jalap; and one of them goes so far as to affirm that it is in his own observation, the *regular effect* of castor oil to bring on a formed attack of Gastro Enterite, by abuse of language still spoken of occasionally as fever.

The Broussaians seem to think the charge of "Electo Empiricism" a very severe and cutting imputation upon us. We are willing to acknowledge its justice; nay more, we are proud to avow the principles implied in the phrase as our constant rule of action. We accept of no man for our master; we follow no leader but truth. Under her banner alone we range ourselves. The knowledge of the effects of every remedial application was derived from experiment; most frequently from accidental observation; sometimes from inquiry suggested through a noted analogy; never, in no one instance, directed by physiological or a priori reasoning. This is equally true of all remedies whether internal or external, whether an-

cient or modern, as far as their introduction is matter of record. What led to venesection as a cure for inflammation? Can any one attribute its original employment to the force of argument, to the result of a logical discussion upon processes and changes altogether unknown? What course of investigation upon the qualities of cinchona, whether mechanical or chemical, would have guided us to the expectation of its febrifuge effect? So much for empiricism, the very essence of which is to follow the dictates of an enlightened experience, and in this sense we are Empirics.

With respect to the Eclectic method, what other can safely be pursued by an intellectual man? For our parts we will abide by it to the very best of our ability, and while we regard with deference the great names which our profession has from time to time ranked among its cultivators, we find in the very differences of opinion and practice which they entertained, the most solid reasons for the exercise of the invaluable privilege of judging for ourselves on all disputed points. Common sense, as well as sound science, will refuse to consider any other *test* of the correctness of any medical doctrine, or system of doctrines than its ultimate success—relative or absolute. There is no other, and to this we must resort.

Of thirty-seven severe cases of continued Fever of which Hippocrates has left us the history, twenty-one died—more than half the number. The average number of deaths in St. Thomas' Hospital, London, during the fifty years from 1764 to 1813 was not more than one in fifteen. At the Bath United Hospital 1 in 20. In Dublin, which has suffered more continuedly from Epidemic Fever than any other great city of Europe, the mortality of the Fever Hospital has been as follows, from 1804 to 1812, in 1 in 12; 1812 to 14, 1 in 15; in 1815, 1 in 20. The mortality of the Edinburgh Royal Infirmary, on an average of the 10 years to 1818 was 1 in 16. The mortality of the Hotel Dieu in 1822, was 1 in 6.82; in the Charite 1 in 5.53; at Lyons 1 in 11; at Montpelier 1 in 10. Now the departures from the Physiological plan are certainly far greater in the perturbing system of the modern English, than in the nega-

tive practice of the ancient Greeks, or the Medicine Expectante of France, during the half century preceding the birth of the illustrious Master. How then are we to account for the vast difference in proportional mortality in the specified cases?

It is obvious on the most cursory examination, that if the perturbing plan of treatment is so erroneous as to deserve all the epithets heaped upon it of late, the merely putting a stop to it must produce an immediate alleviation of the instances of disease which occur—a notable diminution of mortality, and a difference in the length of duration of the attacks. We have a right, I say, to expect these advantages unequivocally from the changes insisted on. Have they been conferred on us? If so, they are not yet known or observable; and such wonderful results could scarcely by any possibility be disguised, hidden or denied. On the contrary it is alleged by Prus that while in 1819 his colleagues Vardy, Desgenettes and Pierre were losing 1 in 12, 1 in 22, and 1 in 18, the mighty Magician, Broussais himself, permitted the demise of 1 in 8. We shall not, however, demand any special degree of success, but we must require that the French practitioners be able to prove an equal amount of alleviation of the evils to which frail humanity is subject, an equal chance afforded to the sick for cure and prolongation of life with their European neighbours and their transatlantic brethren. Then, and not till then, will we consent to receive lessons from them in Therapeutics.

The practitioners of the Southern and Western sections of these United States, are as much in the habit of observing and managing Fevers as any body of their brethren in any part of the world. In regard to their proportional success in their treatment, they yield to none. They follow, with very, very few exceptions the *perturbing* system, and some of them, confessedly not the least estimable or successful, to an extent of which the French adherents of Broussais, or of the old medicine expectante, can form no idea.

We will not then submit to be taught by men who not only have no better fortune than ourselves in practice, but who, taking even their own statements, actually lay no claim to supe-

riority in this respect, and who, therefore, have not the shadow of a right to arrogate to themselves the character of teachers in the Divine Art of Healing.

ART. II.—*On Medical Experience.* By JOHN P. HARRISON, M. D. *of Louisville, Ky.*

IN the whole compass of our language there is not a word of greater latitudinarian construction and of more indefinite import, than the word experience. And this holds with peculiar emphasis, as regards the daily application of the term to medicine. In no branch of human knowledge is there a more frequent employment of the word experience, than in medical science. Experience, it is often averred by those who love to dogmatize on the narrow views they have derived from their own circumscribed opportunities of observation, and experience alone, must be our guide in arriving at just conclusions concerning the causes, nature and cure of diseases. But whilst such advocates of experience so confidently and pertinaciously insist on its exclusive authority in medicine, they seem unable to point out the boundary between experience and routine, and are incapable of attaching any comprehensive and generous intellectual appreciation to a term which appears to convey some talismanic power to their minds.

It has been an unfortunate thing for the progress of the mind in its career of improvement, that language has been so frequently perverted from its high function, as the just interpreter of thought, so as to become the shibboleth of prejudice, or the adopted watch-word of hereditary error. In its legitimate scope of agency language is more than the interpreter of thought; it is embodied mind; it is tangible ideas; it is intellect made visible. But in order to accomplish these exalted purposes language must be employed with a constant and strict reference to its precise and determinate import. Ambi-

guity and imprecision should be banished from the language of every cultivated man, who wishes to impart his thoughts with force, perspicuity, and certainty. Especially, in all discussions on physical phenomena, and in the explication of the laws which govern the material world, should all vagueness and indefiniteness of expression be excluded.

Medicine is a physical science, dependent for its successful cultivation on the same methods as other physical sciences. In medicine the inductive philosophy has achieved some of its noblest trophies. In the several branches of anatomy, of physiology, of pharmacy, of therapeutics, instructed skill, patient observation, and accurate deduction have been the means by which physicians have been enabled to read nature in her operations, to compel her to give up her secrets to the expostulations of her ministers, and to answer the persevering interrogatories of her worshippers. Let poetry in her flights to the highest heaven of invention, deal in words which impart no definite conceptions to the mind, for it is amidst the grandeur of her sketches that she presents to the imagination glimpses of high objects, in a language too indefinite to provoke, and too sublime to allow the scrutiny of reason. But let the terms of medicine be well defined; let the language of physicians be the direct interpretation of the precise ideas existing in their minds; and let the nomenclature of our science be divested of all that cast of expression which, from its ambiguity, has so much retarded the human mind in the road of discovery.

The science of Medicine owes much to experience, but it is an experience at once discriminative and profound. The collected light of ages beams around her path. To the assiduous and enlightened cultivators of our art from the days of Hippocrates to the present age of the world, we are indebted for that fund of facts and rational deductions which go to constitute the science of curing diseases. No one individual, in this age, should arrogate to himself an original experience, completely independent of the past experience of the medical world. Indeed the most skilful in our art pretend to nothing

more than a judicious combination of the experience of the past with their own observations—an application of that knowledge derived from erudition with that acquired by personal inspection, and a careful analysis of the opinions of their predecessors, so as to build upon it a manly and self-sustained judgment which shall enable them to disentangle the perplexities of disease, and to send a discriminating eye over all those details of medical practice which are so apt to embarrass and confuse weak minds.

Each physician must go through a discipline in the school of knowledge; each one has to advance from ignorance to wisdom by slow and measured steps; no man can, per saltum, reach the pinnacle of medical science; he might as well attempt to construct a dome without foundation. All valuable acquisition is the fruit of toil.

Dii laboribus omnia vendunt. Acquisitions collected with labor, are to be carefully preserved, and judiciously arranged. Out of this store, which a zeal for improvement, and a just ambition of distinction, have urged the individual to accumulate, he produces the treasures of mental riches. But without a correct conception of the great ends of our investigation—without a rational view of what we intend to accomplish, how shall we ever profit by the opportunities afforded for personal experience?

The subjects of medical knowledge are so varied—they lie scattered over such a vast field of inquiry, and are of such difficult investigation, that none but a mind disciplined by previous habits of study, and well grounded in those elementary truths which prepare it for fruitful labor, can surmount the obstacles which hinder its advancement in the road of medical science. A perception of analogy and relation lies at the foundation of all our intellectual attainments. It is this which constitutes the mental process by which we compare, reason, and judge. Now, if our minds have never been imbued with the best methods of exercising this perceptive faculty; if the objects upon which it should be exercised are not made familiar by a previous frequent reference to the opinions entertained by

others on the points we intend to investigate, how shall we ever be able to make a proper discrimination of the analogies and relations—the contrarieties and resemblances, existing in the subjects offered to our examination. By learning to measure our strength before we rush into the arena we shall be surer of success in the contest which awaits us, we must regulate the action and measure of our faculties by a careful training, and thus we prevent them being wasted in unproductive and abortive efforts. To this end choice and unity of purpose, as well as perseverance and method, are required. We must, by our choice, determine what shall be the subject for our investigation, and by a unity of purpose economize our faculties, and increase their strength by concentrating them.

Some physicians, feeling an incapacity for scientific investigation, go on to amass a chaos of insulated facts which they dignify by the term experience. But it is by cautious theory that experience can be perfected. Between theory and experience, understood according to their true meaning, there is neither contrast nor opposition. Who are the men that have given to medicine its chief glory as an intellectual pursuit? They are men who inferred general principles from a sagacious comparison of a great variety of phenomena. And this exercise of the reasoning faculties on the subjects of medicine is true theory. A correct experience, within its proper limitation, is nothing more than the handmaid of theory. Between an experience enlightened and guided by theory, and an experience gross and undistinguishing, there is the same difference as there is between the philosophy of Newton, and the matter of fact man that labors in the manufactory where the instruments are prepared by which the heavenly bodies are seen. To the first belong penetration, analysis and judgment—to the second there attaches only a blind routine which supersedes the exercise of the rational faculties.

I shall endeavor in the further discussion of the subject, in as brief a manner as possible, to point out the sources of false medical experience, and enumerate some of the many follies which it has engendered and propagated; and in the next place

notice the best methods of acquiring a true medical experience. and expatiate a little on the immense benefits to be derived from such a source of knowledge.

1. *False Medical Experience.*—The principal sources of the many "false facts" by which every department of human knowledge is more or less vitiated are the following. 1. Superstition. 2. Credulity. 3. Fallacy of testimony. 4. Ambiguity of language. 5. False theory.

It is difficult to bring errrors into any regular classification. Truth is one and the same, but error puts on infinite and ever varying aspects: from its essential nature it appears to have no limits and no end. But the limits it has not in itself, it has in the nature of the objects about which it is conversant, and from the constitution of the mind. In medicine there are a certain number of outlets by which the mind forsakes the straight way of truth. These have been indicated by several respectable writers, such as Zimmerman in his work on Experience in Physic, Paris in his Pharmacologia, and Sir Gilbert Blane in his Medical logic. I have selected from them the principal sources of false medical experience, and shall as succinctly as possible point them out.

1. *Superstition.*—Ignorance is the foodful nurse of that belief which reposes in security on the extraordinary and supernatural agency of remedies in themselves inert and ineffective. In the twilight of mind objects assume an adventitious appearance. Fancy peoples the vicinity of ignorance with endless delusions. Instead of penetrating the true causes of phenomena and tracing out the harmony which marks the beneficent arrangements of nature, the mind infected with superstition goes out in eager quest of strange and anomalous fortuities, and is ceaselessly employed in conjuring up phantoms, which terrify and rule the soul.

The history of all human science bears mournful evidences on its ample page, of the corrupting influence of superstition. Even among the Greeks those who first attempted to assign the natural causes of thunder and lightning were condemned as the enemies of the gods. The fate of Galileo is

familiar to all. Medicine has been deeply tinctured with a reliance on superstitious remedies. Among savage tribes of mankind the whole art of curing disease consists in the practice of ridiculous, disgusting mummeries. Among the Chilian Indians the only mode of relieving the sick is by blowing around their beds, so that if a doctor can blow well, he is sure to obtain a good business. But even philosophers have manifested superstition in matters relating to the cure of diseases. Boyle asserts that he saw a sympathetic powder effect a speedy restoration to health, and he places great reliance on the thigh bone of an executed criminal for the cure of dysentery. The great mind of Lord Bacon was darkened by a superstitious belief in charms and amulets. An extraordinary virtue was attached by Soranus to the honey that was produced by bees that lived near the tomb of Hippocrates. Among the Druids of Gaul and Britain, who were both priests and physicians, the misletoe, gathered with a gold knife, when the moon was six days old, and consecrated by incantations, was esteemed an infallible antidote to poisons, and a preventive of sterility. Galen is considered as the author of the anodyne necklace, so long famous among the great and little vulgar of England. He asserts that if threads are tied about the neck of a viper so as to suffocate it, and afterwards tied around a patient's neck, they will cure all kinds of tumours arising on it. The royal touch of England, and the use of amulets and charms sprang from the same source. But as the present enlightened state of medical knowledge has dissipated, among professional men, these false and erring lights of superstition, I will no longer dwell on this point, but proceed to notice the agency of the next enumerated source of false experience in medicine.

2. *Credulity.*—This vice of the mind is nearly allied to superstition; ignorance is the mother of both. But though derived from the same parent, they differ in some essential features. Credulity gives credence to propositions unsustained by sufficient data. The credulous man believes extravagant accounts of the virtues of medicines, perhaps in themselves potent, but not possessed of all the virtues ascribed to them. Ne-

velty spreads a bewildering gloss on remedies, to the eye of him who has not a sufficient maturity of judgment to resist its fascinations. A warm and animated zeal for any cause, unless that zeal is chastened and regulated by a severe discipline of judgment, is sure to mislead the mind. Quod volumus, facile credimus, is as true in medicine as in the ordinary pursuits of life. Our science is filled with convincing illustrations of this position. The discoverer of every new remedy asserts that he finds in it an epitome of the whole Materia Medica. The metallic tractors of Perkins, the Animal Magnetism of Mesmer, the elixir of life of Paracelsus, the alkahest of Van Helmont, and the Panaceas and Catholicons of the present day, all have derived their source from fraud operating on credulity. Even where no sordid considerations influence the propagator of a revived, or newly discovered remedy, the ardent enthusiasm of the experimenter urges him to great extravagance in the commendations he bestows on it. Baron Storck greatly overrated the remedial power of the Narcotics upon which he made so many experiments. There is no prominent remedy in our catalogue of agents for the removal of disease, but what has been at one time surrounded by a mist of panegyric. And how many inert substances have been eulogized, that are now quietly slumbering in the tomb of forgetfulness! Opium, Digitalis, Mercury, Antimony, and the Bark are the most effective weapons of medicine. Each of these have been injured in their legitimate character, as medicinal substances, by the indiscriminate encomiums lavished upon them. The most enlightened physicians now agree with the illustrious Sydenham, "that the practice of physic chiefly consists in being able to discover the true curative indications and not medicines to answer them; and those that have overlooked this point have taught empirics to imitate physicians."

Credulity delights in going in constant search of anomalies and novelties. When a new remedy is suggested for some incurable malady, such as Prussic acid for tubercular consumptions, or scutellaria for hydrophobia, the credulous minded physician does neither hesitate, nor suspend his judgment, but rush-

es to a precipitate conclusion, and upon some very inaccurately observed isolated case proclaims his unlimited confidence in the article. It is in the rank soil of credulity that quackery flourishes, and sends forth its luxuriant branches.

The *Eau Medicinale d'Husson* not many years since was implicitly relied on for the cure of gout, and the case of Sir Joseph Banks was quoted in its favor. What now is the just opinion attached to that nostrum by the most learned men of our profession? Let any one read what the profound Parry says about it and he will at once be convinced of the injurious agency of the remedy, in preventing regular paroxysms of a constitutional disease, and thus exposing the patient to the imminent peril of apoplexy, or palsy, or some other severe attack of disease.

The doctrine of Signatures in the choice of remedies sprung from the same source, an excessive credulity. Fox's lungs were given to cure asthma, because the animal was capable of running long before he became exhausted. Turmerick, from its yellow colour, was administered as a sure cure for jaundice. Other analagous instances might be given, but I must hasten on to the next source of false experience.

3. *Fallacy of testimony.*—It was the remark of a celebrated teacher of medicine that there is in our science more false facts than false theories. Paradoxical as this may seem, its truth is established by the most comprehensive and accurate observations which can be made on the wide field of practical medicine. Wherever we turn our eyes, whatever part of the field we may contemplate, there rises before us a thick cloud of false facts. There is no department of our Art so overrun with the fruits of false experience as the Materia Medica. The best and most approved authorities on that branch of medicine have made this statement. Whence originates this lamentable state of things? Why is it that this valuable department of our science should abound in such uncertainties, and be oppressed by such a load of useless remedies? It is clear that in order to ascertain with precision the veritable operation of any remedy, three conditions are required. 1st. Whether the patient is really afflicted with the disease for the cure of which the reme-

dy is to be administered. 2nd. After the remedy is given whether or not the patient is cured at all; and 3d. whether, if cured, it was the remedy which cured him, or he got well in despite of it.

The great importance of Sydenham's remark is now more distinctly perceived. Unless the physician understands the nature of the disease and from that knowledge gathers the indications of cure, of what avail is it that he has a long catalogue of medicines to administer? Here the paramount consequence of a true theory reveals itself. It is upon the well established basis of a correct pathology that the triumphs of our science are achieved. Here charlatanry stands rebuked, by the well sustained principles of scientific investigation; here it "loses, discountenanc'd, and into folly turns." Very frequently reputed cures are discovered to be only a partial suspension of the onward career of a malady. The dominant control exercised by imagination over the excitable hopes of a patient suffering under some chronic affection operates a very great influence on the body. It is in this way that the supposititious cures of Mesmer, and Perkins were accomplished. It is in this manner that charms and amulets have sometimes suspended the progress of a fatal disease. In many instances of this kind the patient has no real disease, but is under the dominion of a gloomy and fantastic fancy. The case of stone supposed at the time to have been cured by Mrs. Stephen's remedy, for which Parliament liberally rewarded her, is in point. In that instance we have a verification of the fallacy of medical testimony. The patient was examined by medical men of the highest reputation, and upon their declaration of his being cured, the reward was granted. But the man still laboured under the stone, only it was concealed from detection by the sound, by its being invested in a sac of the bladder. The reputation of other nostrums has been reared on a similar perishable foundation. The fallacy of testimony is forcibly illustrated in the asseverations of some of the older writers, respecting the presence of globular mercury in the bones. The solemn march of the germs of the future animal in the

semen of a ram which drew forth such wonderment from Boerhaave, stands on the same grounds; and so the contagiousness of bilious remittent and yellow fever, is advocated by those who place too much credence in fallacious evidence. Fordyce attempts to prove that common remittent fever is contagious, and the advocates of the contagiousness of yellow fever are found in the highest ranks of the profession. Coincidence is taken for causation—the *post hoc* occupies the place of the *propter hoc* on such occasions. In consequence of that prevailing link of sympathy which binds together men in society, there spreads at times a sort of universal assent to any, however extravagant, statement. Thus the mind of each one seems predisposed to admit without due examination, the slightest proof as corroborative of the remedial efficacy of any new article of the materia medica. Physicians partake of this feature of mind with other men. They too are easily satisfied with the testimony afforded in substantiation of the virtues of a novel remedy. Hence it is that so many in our profession stand ready to

"Catch, e'er she fall, the Cynthia of the minute."

Hence it is that medicine seems more like a magic lantern, where the scene is perpetually shifting, than like the sober light of the great orb of day.

4. *Ambiguity of language.*—This is an exuberant fountain head of error in every species of intellectual inquiry. Accurate language subserves, in our profession, three important purposes. First, as an instrument of intellectual analysis. Secondly, precision of expression affords a medium for distinctness of thought, like a clear atmosphere gives the eye the best opportunity of perceiving the exact outlines of bodies. Thirdly, accuracy of language is the only sure interpreter of perspicuous views; for imprecise and indefinite language is always the result of a clouded and confused comprehension of a subject. It would be well for every young man, who is preparing for the practice of medicine, to read over most carefully Locke's admirable chapter on the necessity for clear and accurate expression as the elucidation of truth. For says that

great man, the want of a precise signification in words is the cause of very obscure and uncertain notions. There are two words of great currency in the profession which will afford an apposite illustration of the ambiguity of medical language. These words are *Congestion* and *Sympathy*. It is apparent that the words congestion and sympathy are generic; that they are similar to the word inflammation, which comprehends several species of diseased action. Whenever, therefore, such generic words are employed to convey ideas relative to a species, or a variety, they are misapplied. Thus, without pausing now to examine the correctness of the pathological views involved, when we read of congestion of the brain, or liver, or lungs, do we acquire from such indefinite language any exact and precise views of the condition of the affected organ? Such fashionable words do little in conveying accurate impressions to the reader's mind. Like general reflections they can never elucidate practical points in medicine. The writer must enter into detail, and do much by circumstances. Nothing can elucidate but what is pointed. Every remark must particularize and specify. The eye may take in a large range, but the mind is affected by inspection. We must descend from the brow of the hill, and come down to certain spots and objects. The science of medicine can never be much benefitted by such general, diffusive, and indefinite language as that employed by many writers. Thus when they have designated a fever congestive, or when they have ascribed the origin of a disease to sympathy, or have traced all attacks of febrile affections to inflammation in the stomach, they seem to imagine that their task is finished, and that they have completely satisfied the reader as to the pathology of the disease. But what precise views does he derive from such language? The great advantage, to the writer at least, of such language is, that it saves all further difficulty in his description of a disease. But the reader is completely beclouded in his apprehension of the pathological condition of the suffering organ, when he is told that it is congested. What is congestion, he is ready to inquire? This I will endeavour to answer before this discussion closes.

5. *False Theory.*—It is a well established fact that some of the most erroneous theories in medicine have originated from men, who professed to despise theory. And those men who are most addicted to expressions of contempt against reasoning in medicine, are the most disposed to indulge in crude speculation. And the cause of this obvious; it springs from a too exclusive reliance on their own individual experience, apart and independent of that accumulated wisdom of ages to be found in books. "To think is to theorize," is a proposition not to be successfully controverted. So deep in man's intellectual nature is laid that disposition to account for phenomena, on which all philosophy is built, that he cannot be induced to forego the gratification it affords, under any condition of his being. The employment of reducing truth to its element is one of the most gratifying and useful occupations of the mind.

The luxuriant growth of our science, from the multitude of facts which have been collected by its assiduous cultivators, demands that comprehensive and accurate principles should be deduced from its many and insulated particulars. It is the object of correct theory to reduce the multifarious appearances of disease to simplicity and order. In the present state of our science there is an imperative necessity for the exercise of an inquisitive and powerful reason, to remodel and arrange the facts that are already ascertained, and to trace up the analogies which run through diseases to some general principles. Thus we shall be enabled to make a discriminating survey of the miscellaneous variety of particulars which are placed beneath our observation. To frame a correct theory we must keep in mind the remark of a distinguished medical teacher—"Medicina neque agit in cadaver neque repugnante natura aliquid proficit"—that medicine will neither act on a dead body, nor will it act on a living body, in a way contrary to the laws of the animal organization. It is from an utter forgetfulness, or contempt, of this essential principle that the science has been infested by so many chemical and mechanical theories. The humoral pathology rested on a fallacious and shallow conception of the laws of life. The nervous system, which con-

stitutes the living being in contradistinction to the organized dead mass of animal matter, was overlooked—its structure was disregarded, and its laws unobserved.

But as the sun of medical science has ascended, the mists which hung around the path of improvement are disappearing. In exact proportion to the speed of correct views on the anatomy and physiology of the body has been the retreat of the humoral pathology. Instead of physicians speaking in unintelligible language of lentor, and alkalescency; of fermentation and concoction, and of virus floating in the blood, we hear of correct statements of symptoms, and of the actions going on in the solids. What has humoralism ever done for medicine? It has retarded its march, it has shut up the avenues to its successful cultivation, and deteriorated the genuine spirit of correct investigation.

To particularize. Scrofula was once attributed to a vitiation, or depravity of the fluids, and the treatment was empirical in accordance with such an erroneous pathology. Since the renunciation of this erroneous pathology, and the adoption of a correct view of the disease as one of the solids, the treatment has been established on scientific principles. And so it has been with gout, rheumatism, syphilis, small-pox, and several other affections.

Dr. Armstrong's popular doctrine of congestion is founded on wrong views of anatomy and physiology, and is therefore untenable. Indeed at page 288 of his work on Typhus, under the head of common continued fever, we have an admission that congestion and inflammation are the same. "The action of one artery," says he, "I have never known greater than that of another, and what we call increased action is, I suspect, merely increased accumulation, and what we call increased determination is, I also suspect, merely an increased volume of the vessels, arising from an impediment to the return of the blood from the quarter to which those vessels lead." And at page 159 of his Essay on Pulmonary Consumption he makes the same admission of the oneness or identity of congestion and inflammation. "It necessarily follows that when the ener-

gy of the heart and arteries is much diminished, that they cannot maintain the natural current of arterial blood, and of course a proportionate accumulation takes place in the veins; and this venous congestion appears to load and stimulate the capillaries of the arterial system by retarding the return of the blood through them." Four widely different pathological states of the system may be mistaken for congestion.

1st. Sympathetic derangement from worms; or ingesta in the alimentary canal; or from uterine irritation giving rise to hysteria.

2d. Constitutional irritation from surgical operations; loss of blood; or severe burns.

3d. Suffocated excitement, or such an intense degree of action as to overwhelm the powers of life. This often occurs in yellow fever.

4th. Collapse of the powers of life in the last stage of fever. Dr. Potter in his edition of Armstrong admits that "the symptoms so formidable at the close of fevers are often nervous only, and the seeming congestion of as much importance as they are in hysteria."

The French pathology of fever is amenable to a just objection on account of its generality. It is a recommendation to many minds, not disposed to scrutinize, that such pathological views involve no laborious process of thought, and demand no extensive examination. They are light and portable, easily transferred from the author's page to the reader's comprehension. The memory alone is exercised in their reception. To remember that every case of fever is nothing but congestion in the liver, or a gastro enteritis, is quite a compendious mode of arriving at a knowledge of the pathology of a class of diseases, whose history, nature and practice have tasked the mightiest intellects of our profession. And then the by-cut; the saving of thought; and the fallacious facility it gives to practice! The allurement held out by the simplicity of Brown's system enraptured for a while the medical world, especially the younger members of it. Dr. Rush's unity of disease rose with overpowering lustre above all other views of pathology, but it has long

since fallen "like an exhalation in the night, to rise no more" unless Broussais' gastro enteritis, or Armstrong's congestion, assume its guise, and limp after it in awkard imitation.

3*d. Methods of acquiring true experience.*—There are two main sources opened to us for the attainment of true experience in medicine. The one direct, emanating from our own personal observation; the other derivative, flowing from the testimony of others. It requires but little reflection to convince the most obtuse mind that the last is the most copious and opulent source; that standing on that elevation we see centuries placed beneath the eye, little names are obscured, things are adjusted to a true point of comparison, and the glaring colors of present fashionable doctrines are effaced. To this point nothing should ascend but the spirit and true quality of medical truths. The science of medicine may be divided into principles and practice. Although for the purpose of a more easy and perspicuous discussion, such a division may be adopted, yet as a useful and necessary art which is constantly operating on the dearest earthly interests of society, such an arbitrary separation between the principles and practice should not obtain. True experience is founded on a close union of the two; for without principles medicine is but a mechanical trade, and without practice those principles degenerate into idle speculations. What is so frequently called experience deserves not that appellation. *The wise only profit by experience*, is an adage that receives its most emphatic illustration in medicine. A man ignorant of the fundamental principles of the science, and incapable of reasoning in a correct manner, may grow grey in the practice, and remain ignorant of the most important truths. How often the hoary head of inveterate error occupies the chair of authority, which wisdom alone should fill. The most profoundly ignorant of medicine are often those who have paced along the deep worn path of routine for near half a century. A thorough indoctrination of the principles of medicine; an extensive, accurate, and profound medical erudition, are absolutely demanded to qualify men to acquire knowledge from their own observations. In vain are facts pre-

sented to the eye of one ignorant of the principles of our science; he is not capable of perceiving their import nor of deducing from them those corollaries which will guide him in future cases. There are no two cases of disease which bear an exact veri-similitude to each other in every respect. The endless varieties observed in different patients suffering under the same malady arise from difference of constitution, diversity in modes of life, from the distinct nature of the exciting cause, and from the peculiar emotions which may at the time agitate the mind. To prescribe therefore the same remedies for the same disease without a due inquiry into the state of the system, merely because they were successful in another patient, is rank empiricism. A perception of analogy and contrariety must guide the practitioner in all new cases. Possessed of an accurate knowledge of the structure and functions of the healthy human body, and having acquired from his derivative experience a just conception of the nature, symptoms and treatment of the species of disease, to which the case under his observation belongs, the physician is well prepared to administer, not for the name of the malady, but for the state of the system.

There are three modes in which the mind acts in its acquisition of true experience, from personal observation. By the spirit of observation it collects the elements of judgment; by the force of its judgment it incorporates those elements, and by its creative energy it models and realizes new and felicitous combinations of thought. The spirit of observation is the messenger of reason, sent out before to explore and prepare the way. By it we penetrate into the phenomena of nature, and by the judgment and creative power of the mind we appropriate all, that is gathered by observation, to the formation of a calm and contemplative turn of thought, which is not easily misled by the glare of novelty, nor seduced by the erring lights of a false experience.

By what preparatory steps, then, shall the mind be best qualified to profit by clinical observation? In answer to this question I reply, that it is, 1st. By a careful sedulity of effort in obtaining correct views of the anatomy and physiology of the hu-

man system. 2d. By ascertaining from books and lectures an accurate knowledge of remedies. 3d. By deriving experience from the testimony of judicious writers and lecturers on medicine.

The foundation of all correct medical and surgical knowledge is anatomy. Disease is the product of disordered action in the living moving powers of the body. Disordered action cannot be understood without a knowledge of healthy function; healthy function cannot be understood without a correct knowledge of structure; and structure cannot be understood without investigating the anatomy of the system. The disordered movements of function can only be controlled by touching the springs of life. From the composite mechanism of the human body, and from the concealment of that mechanism from common observation, it requires an assiduous examination of the interior structure of each organ, vessel, nerve and tissue, in order to understand the relative situation and connexion, the nature and operation of the various parts of the frame liable to disease. In the pervading agency of the circulation, and the dominant influence exerted by the nervous system on all the actions of life, we see the immeasurable importance of a correct knowledge of these two great systems, in order understandingly to administer to cases in which they are involved in disease. And what disease is there in which they are not involved? Let one illustration suffice. When the liver is diseased there is often pain at the top of the shoulder. How is this to be accounted for by a man ignorant of anatomy? Such a man knows nothing of the right phrenic nerve which sends a branch to the liver, nor of the third cervical nerve, from which the phrenic arises, distributing branches to the neighbourhood of the shoulder; thus establishing a nervous communication between the shoulder and liver. Disease of the liver is sometimes treated by ignorant physicians as rheumatism of the shoulder.

To the surgeon anatomy is all in all. It is what Bacon called knowledge in general, it is power in every sense of the word—power to prevent, power to alleviate, and power to save. Without anatomical knowledge the surgeon is like a mariner

on the sea without compass, chart, or the stars of heaven to guide him in a stormy night. The great improvements in surgery have invariably proceeded from the best informed anatomists. Surgery has risen to the certainty and perfection of its present condition by the cultivation of anatomy. John Hunter, the light and glory of English surgery, was the greatest anatomist of his age. Philip Syng Physick, the light and glory of American surgery, received his elementary knowledge in anatomy from Hunter.

The diffusive lustre of French Pathological and minute Anatomy, has shed the rays of a better philosophy on clinical experience. Commencing with the powerful and penetrating genius of Bichat it has gone on to a more exalted position of usefulness. The physician who is ignorant of the researches of Bichat, and of the other authors who have written on minute and pathological anatomy, is ill prepared for profiting by his own observation. Such a one is destitute of the true elements of a correct clinical experience. He is like a man who attempts to construct an edifice without a knowledge of the rules of architecture; or like one who essays to solve an algebraical problem when he has omitted one of the elements of the calculation.

It is absolutely required as a preparation of mind for the obtainment of a sober experience of the remedial power of the different articles of the materia medica, that the physician should be versant in the history of the science. History is philosophy teaching by example. When we read of the numberless articles which have been obstruded on the medical world as rare and effective remedies in many diseases—when we read of the undistinguishing eulogies passed on inert medicines, and learn the adventitious fallacy connected with the reputed experience which gave them birth, we are made wise by such examples, and acquire that philosophical wariness of mind which leads us to listen with some considerable degree of scepticism to the accounts given of any new and extraordinary medicine. The history of the materia medica is little else than a history of false experience. The stream of

time has washed away the dissoluble reputation of the great majority of articles once held in high estimation. A few are seen standing erect in the flood, and upon them is affixed the sure seal of an approved experience.

Amidst the wide spread triumphs of the art of printing it were a superfluous task to attempt any commendation of the inestimable value of that transmitted experience, which we find recorded in books. Medicine is as deeply indebted to the art of printing for its present elevation and usefulness as any other tentative and progressive branch of knowledge. Cicero said that the man who knew nothing of the past by reading was always a child. And so it is in medicine, the physician who is uninstructed in the history of his science, and who expatiates on his own narrow field of observation, without deriving any light from his predecessors, or cotemporaries in medicine, is likely to remain a child in true experience all his days. To be sure he may have all the confidence which a true experience should alone give, yet is the blind assurance of ignorant arrogance, not the firm dignity of conscious acquirement.

True experience does not always grow with the progress of years. In many instances advancing years but confirm the decisions of prejudice, and add strength to the presumptuousness of ignorance. Neither does the multitude of patients which a physician may be called to attend necessarily enlarge his views, or add to the vigor of his judgment. It is not the number of cases which a physician sees, but the degree of careful analysis which he makes of those which are submitted to his inspection, which constitutes true experience. What is it that distinguishes the observing traveller from the undiscerning one? They both visit the same spots, and yet one, will only tell you of things which interest you not, whilst the other by leading you through the region of life, and manners, will invigorate your intellect, and send a refreshing influence through every recess of thought. And what is it that made a Newton see an apple fall with a different eye from the millions who had witnessed the same phenomenon before? It was the power of perception, which dwelt in his inquisitive and penetrating

mind, that separated him so far from the generality of mankind. And what is it that distinguishes the physician from the nurse, but the disparity existing in the degree of their mental cultivation? The nurse is in possession of the same senses by which the phenomena of disease are discerned as the physician, and yet the pure emanations of an enlightened reason and a true experience, are the product of the observations, and of the theoretic views of the one, whilst the gross and vulgar apprehensions of the other seem more like the limited, instinctive perceptions of the lower animals.

From what has been said it appears that true experience is to be acquired by a careful avoidance of those sources of error, fallacy, and misapprehension, whence false experience springs, by a well grounded knowledge of the elementary branches of medical science, and by the exercise of a discriminating observation upon the phenomena submitted to personal inspection.

But if the physician permits his mind to be preoccupied with any shadowy and airy notions of congestion, or any other universal all-embracing, all-explaining, and at the same time vague and indefinite, a priori view, of disease, in vain opportunities, the most ample and diversified of acquiring a correct experience are presented.

Such a one sees his predominant image of mind reflected in every symptom—all phases of disease are but the variegated aspects of the ruling figment of his mind, and like a man with a calenture of the brain, he peoples vacuity itself with the creations of his prolific fancy: or with Wirdig he runs into the extravagance of endeavoring to explain every thing in heaven and earth, and under the earth by his favorite hypothesis. Thus that erring philosopher asserted of magnetism, "Universa natura magnetica est; totus mundus constat et positus est in magnetismo, omnes sublunariæ vicissitudines fiunt per magnetismum; vita conservatur magnetismo, interitus omnium rerum fiunt per magnetismum."

Such a habit of mind converts knowledge into prejudice, and limits every intellectual exercise within the narrow boun-

daries of a crude and indigested hypothesis. Our ideas should be daily subjected to new developements, and knowledge acquired by past observations must be constantly quickened and made prolific of new trains of thought by incessant efforts of improvement. Let us beware of a desire to subsist all our lives on the acquisitions of a few years study and experience, let us not exist in a state of mental petrifaction—preserving a lifeless form of medical experience, without the animating spirit of discovery, but uniting the acquisitions gathered from the past with the accessions of each revolving day, we shall rise higher and higher in the scale of improvement, and become more firmly fixed on the ground of true medical experience.

Art. III.—*The nature and cure of the Indian Cholera.*

[Note. The vast importance of all information relating to a disease which has spread such havoc and desolation over Asia and Europe, and whose introduction into our own country seems to be fearfully apprehended by many, will be our apology for transferring to our pages the following communication, which we copy from the 1st Number, (vol. 5,) of the Boston Medical and Surgical Journal "Throughout the whole diversified objects of medical science, there is perhaps none of so much interest at the present moment as the Asiatic or Epidemic Cholera. Its extensive diffusion in the course of a few years over a great part of the eastern world, its almost unexampled mortality, the appalling swiftness with which it brings its victim to the grave, and the mystery which continues to hang over the cause of its origin and progress,—have for some time rendered it an object of attraction to every scientific inquirer who confines not his view to what passes within the narrow circle of his immediate observation."*]

IT behooves every medical practitioner to watch with eagle eye the progress of this dreadful malady, and to treasure up in

* Edinburgh Med. and Surg. Jour. July, 1831.

his mind every incident in its history which may aid in forming philosophical views with regard to its treatment. It will not therefore be with indifference that the reader will peruse the following remarks, which we derive from a late No. of that able London periodical, "the Englishman's Magazine."

The Indian or Spasmodic Cholera is a disease quite distinct from the Bilious Cholera which is common in this country and in Great Britain in hot seasons, and the author commences by pointing out the distinctive characters of these two diseases.

The generic appellative, *Cholera*,* is radically derived from a Greek word signifying bile. The title was appropriated in consequence of one of the most prominent symptoms being either a redundant flow of bile into the intestines, accompanied with evacuations of a *bilious fluid*, characterizing *Bilious Cholera;* or a total absence of bile in the intestines, with evacuations of a *watery fluid*, characterizing *Spasmodic Cholera*. These symptoms are so completely opposed to each other in the quality of the fluid discharged, that attending to them alone will in general preclude the possibility of confounding the two species.

In this country, the more severe forms of Bilious Cholera usually occur in the latter end of summer, or the beginning of autumn. At these periods, the extreme heat of the sun stimulates the liver to an increased secretion of bile, which, flowing in excess through the biliary canals, accumulates within the intestines. In the condition of health, a certain quantity of bile is a necessary adjuvant to the perfecting of the digestive process; but, like other natural secretions, when above or below the salutary measure, it becomes the cause or the indication of diseased action.

The opening symptoms of Bilious Cholera, are simply the efforts of nature to expel the superfluous bile from the system. The patient complains of nausea and universal languor, to which retchings and evacuationsof a bilious fluid quickly succeed. There is also thirst, restlessness, and pain. In many cases, occasional cramps are experienced in the limbs, resulting doubtless from the irritating effects of the bile upon the inter-

* Cholera *Morbus*, the vulgar name, is an absurd pleonasm.

nal surface of the intestines. Of spasmodic action, induced in distant parts of the body in an analogous manner, there is conclusive evidence. In children, convulsions are often excited by the irritation of worms, or indigestible matter lodged in some part of the alimentary canal.

A knowledge of the nature and seat of a disease, will lead to a judicious plan of medical treatment. The course to be adopted here is very obvious. There are two objects to be attained. One, to dilute and remove the irritating fluid from the bowels; the other, to allay the thirst, pain, and spasms. The first indication is followed in the free exhibition of mucilaginous liquids, and the use of an occasional purgative; to compass the second, anodyne medicines and refreshing drinks should be administered. When properly treated in the beginning, it is very seldom indeed, under ordinary circumstances, that cases of Bilious Cholera prove fatal. The disease is acute for the time it endures, but generally it is subdued in three or four days, and the patient recovers with nearly the same rapidity that he fell sick. There are periods upon record, however, in which the malady assumed a more dangerous tendency, in consequence, probably, of a peculiar constitution of the atmosphere with regard to heat, moisture, &c., or properties unknown. In 1669, Sydenham describes the Bilious Cholera as unusually prevalent and severe, carrying off many victims in the space of twenty four hours.

Spasmodic Cholera is strongly distinguished from the preceding species by the intractable nature of the malady itself, and by the greater intensity of some symptoms common to both, as well as by the features, which confer upon it a distinct specific existence. Spasmodic Cholera is practically unknown in England, but in India it is indigenous. The attack is usually most insidious and sudden. Persons who may have felt perfectly well during the day, in the course of the night, or early in the morning, are seized with a feeling of uneasiness, which is at first rather a general sensation than referable to any part. To this succeed, at irregular intervals of time, a sensation of heat in the region of the stomach, nausea, constant evacuations from the

stomach and bowels of a fluid bearing a striking similitude to water in which rice had been boiled, cramps of the muscles of the fingers and toes, which gradually ascend along the limbs, to the trunk. Finally, the muscles of the chest and belly are included in the circle of spasmodic movement, the more violent symptoms continue to persecute the patient, until his strength is unable longer to sustain them. In the last stage of the disease, he is emancipated from the vomiting and spasms, through the complete exhaustion of physical power. With this change, however, he expresses himself greatly relieved, and he may yet survive for a considerable period, his mind remaining unimpaired amid the wreck of the corporeal functions.

A physician conversant with cases of Spasmodic Cholera, will often be competent to recognize an approaching attack in the expression of the patient's face, before that he is himself conscious of the least alteration in his appearance or sensations. His features seem sharper than natural, and there is an air of repressed anxiety in his countenance. If his attention be called to the fact, he will then perhaps say that he does not feel altogether as easy as in general, but that he cannot account for the impression. The changes which the pulse and skin undergo, in the more obvious stages of the malady, are very remarkable, and will not fail to strike the attention of the most superficial observer. At first, the pulse is rapid, small and weak. When the spasms are established, it becomes, during the paroxysms, imperceptible in the limbs, and for some time before death no pulsation can be detected in these parts. The inability to carry on the circulation in the superficial textures, allows the blood to accumulate and oppress the internal organs. On *post-mortem* examination, the different viscera, particularly the liver and lungs, are found gorged with thick, dark-coloured blood, showing that the eliminating processes of respiration and secretion had been very imperfectly performed. The surface of the body, at the commencement of the disease, is pale, chill, and clammy. In the advanced stages, it is quite cadaverous.

The first case of Spasmodic Cholera that challenged our

professional skill, occurred in Calcutta. The symptoms were sufficiently marked to make a lasting impression upon the mind of a practitioner hitherto practically unacquainted with an enemy so obstinate and so accustomed to triumph.

The patient, Mr. A——, an European, only a few months transplanted from England, was a middle-aged man, of spare, but muscular proportions. The evening previous to the attack, he had been abroad enjoying the society of some friends, and, contrary to his general habits, he had indulged in the rather free use of spirituous liquors. The party separated about one o'clock. Mr. A——, the distance being short, travelled home in a palankin. Upon reaching his residence, he did not retire directly to bed, but took a seat in the verandah "to cool himself." In this state of exposure to the night air, he fell asleep. He slept soundly until awakened, an hour afterwards, by his servant, who reminded him of the propriety of going to bed. Either from the disease not being perceptibly manifested, or from impaired sensibility, Mr. A—— was not then conscious of any morbid affection. After slumbering in bed, however, for a couple of hours, he awoke suddenly with a start, complaining of mental anxiety, and a feeling of uneasiness at the region of the stomach, which he attributed to unpleasant dreams, and to the effects of the wine. But the anxiety increased, and the uneasiness changed into a feeling of burning heat. In the lapse of four hours, evacuations of the portentous fluid, like rice water, followed, from the stomach and bowels, to which were soon superadded distressing cramps, affecting the muscles of the toes. The character of the affection was now evident to the patient himself, though almost a stranger to the climate and its diseases. To afford medical assistance, we were immediately summoned. The summons was quickly obeyed, but, in the interim, the advance of the disease had surpassed our rapidity.

On arrival, we found that the patient had been unable longer to endure confinement in bed. The cramps had extended upwards to the calfs of his legs; they were also simultaneously experienced in both arms. So violent were the spasms, that

he had rolled in torture upon the floor. At the time we entered, he was gathered into a corner of the apartment, and he presented an appalling spectacle of internal agony. His person sparingly concealed in a night-garment, bore, in its spasmodic contractions, a resemblance to the letter S. Assisted by a couple of servants, and by pressing his bent extremeties against the angular walls, he laboured in the extreme exertion of voluntary force to subdue the involuntary action of the rebellious muscles. The expression of his face, at that moment, lives distinct in our recollection; and even there, though seen through the mist of receding years, it is painful to dwell upon.

To convey to the reader a faint idea of the death-struggle then maintained, we would liken Mr. A—— to a traveller, who, falling unwarily upon the tiger's lair, rallies every nerve to secure a temporary respite. His inevitable fate is briefly procrastinated by the convulsive grasp which holds the throat of the savage in momentary subjection. Such was the danger—such the desperation stamped upon the countenance of our patient. His features were sharp and hollow. His teeth clenched in breathless agony. The blood had retreated from his cheeks and lips. His limbs were doubled resistlessly by the remorseless spasms.

In a few minutes, a remission brought partial relief. In occasional snatches of expression, he gave us to understand that he was now able to answer the interrogatories we might think proper to propose, with respect to the inward symptoms. These symptoms were the usual concomitants of Spasmodic Cholera in the intermediate stage of its violence. His stomach felt as if it contained a furnace. The thirst was unslakeable. And so complete was the feeling of exhaustion during the interval of mitigated suffering, that he felt unconscious of possessing the slightest control over the motions of any part of his physical structure.

Medical men have been collectively accused, by the indiscriminating multitude, of defective sympathy towards the sons and daughters of bodily affliction. Habituated, it is said, in their daily avocations, to the appeal of the unfortunate, they are at last led to conceive the heart-pang of the patient to be as un-

substantial as the words in which it is expressed. The charge is untrue. In no class of educated people will there be found a greater proportion of "hands open as day to melting charity," than might be discovered among the members of the medical profession, were their kind acts performed in the market-place, instead of the gloomy recesses of morbid destitution. Men in the general walks of life may annually exhibit their measured benevolence in public places, and shed the infrequent tear of commisseration in their hasty transit through a hospital; but it is the province of the practitioner to do something more than this. It is his duty to linger long with the distressed, to bind in solitude their bleeding wounds, and when hope has departed, never to return, to wear her cheering portraiture, that the abrupt and rugged path leading to dissolution may not too hastily reveal the extremity of danger.

To our patient, labouring under a violent and advanced attack of Spasmodic Cholera, no solid expectation of recovery could be extended. Every means, however, sanctioned by recorded experience, was tried to compass a favourable change. Respecting the final issue, he was himself little if at all solicitous. The immediate suffering, particularly from the spasms, absorbed both sense and soul. He prayed imploringly to be relieved, either by energetic treatment or by death, from the intolerable cramps that threatened to tear him into pieces.

Brief was the interval between supplication and repose. The resources of nature were exhausted in detail. The spasms which had eventually ascended to the body, finally yielded to debility, that proved alike prostrating to the actions natural and diseased. The vomiting ceased to harrass; the pulse was no longer susceptible in the limbs; even the motion of the heart—that citadel of life—was feeble and indistinct. The surface of the body felt cold and clammy like a corpse, presenting on the hands and feet a corrugated and macerated appearance, as if it had been steeped in water for some days. The breath grew chill. The eye was glazed. In this state, notwithstanding, he lived several hours, and then expired without a struggle.

The case of Mr. A—— is a common specimen of the progress of the disease to a fatal termination. It included thirteen hours from the first feeling of uneasiness, until he breathed his last. Cases, however, are continually occurring, in which the duration of the malady extends to twenty-four hours. Some beyond that. On the other hand, many patients are carried off with singular rapidity. In the history of the Epidemic, instances are numerous of soldiers falling in the line of march and dying instantly, as if seared by lightning, without having uttered a previous complaint. Mechanics have perished with their working implements in their hands; the Brahmin, also, at his beads, and the Ryot at his plough.

Before noticing the principles hitherto most successfully pursued in the medical management of Spasmodic Cholera, we shall briefly advert to some of the opinions entertained respecting the causes, remote and immediate, of that terrible distemper. That the reader may comprehend our technicalities of causation, the application of the terms employed shall be explained.

The causes of disease admit of two chief divisions. The *remote* and the *immediate* or *proximate*. It may be permitted for our purpose to illustrate these in the following manner:—A man receives a blow from a stone; the part stricken is bruised; inflammation of the part succeeds. Here the propelled stone is the remote cause of the mischief; the bruise is the proximate cause; and the consequent inflammation forms an array of symptoms, or what is commonly called the disease. In strictness of language, however, we should not call the inflammation, or third stage, the disease itself; it is merely symptomatic of the previous organic change comprised in the second stage. The proximate cause, therefore, is the real disease, the parent of all that follows. Applying this mode of investigation to the English or Bilious Cholera, it will appear that the heat of the sun is the remote cause of the affection; for, during the maximum of its annual range, the heat stimulates the liver to increased action, and its function becomes accelerated. This functional derangement is the proximate

cause; and the augmented flow of bile is merely one of the primary symptoms which, in its turn, gives rise to the secondary train, nausea, vomiting, &c.

As in the two examples just given, every malady will have for its proximate cause either an organic change, or a functional derangement of some part of the body. But in many cases we are not aware of the precise nature of that change or the exact seat of that derangement. It is also often impossible, from the minuteness of the field of research, and the imperfection of our senses, to ascertain by observation or dissection, whether the proximate cause of certain maladies should be referred to alteration of structure or to supernatural action alone. To this order belong a numerous family of fevers, which are developed in the system spontaneously, or through the influence of a deleterious miasm. The difficulty to which we have alluded, accounts for the various controversies maintained of late years, among medical writers, regarding the primary seat of fever. Some attempted to prove portions of the nervous, others, of the vascular system, to be the first link in the chain of febrile transition. A third party, observing that the brain, in persons who died of fever, frequently presented the aspect of recent inflammation, hastily referred the proximate cause to inflammation of the brain. A fourth, for similar reasons, found it in the lining membrane of the stomach. The more enlightened view, however, of the pathology of fever, demonstrates that the inflammatory appearances observable after death, are the *effects*, and not the cause, of the disease. An opinion consequently, has been with reason entertained, that the nervous system, not in part, but as a whole, is the radical seat of the morbid phenomena. The recorded history of spasmodic Cholera, does not reveal to us the reason of the disease changing its character in 1817. Why it should have assumed, at that period, an aspect of extraordinary malignity, has not as yet been satisfactorily explained. The remote and proximate causes of the epidemic are still open to investigation, though several authors have attempted to point them out. Mr. Annesley* seems

* Sketches of the most prevalent diseases of India. London: 1829.

to think that the remote cause was a peculiar state of the atmosphere, with regard to its electricity; that the air was negatively electrical, and that this induced a great diminution of the nervous fluid in the human body, which is in his opinion the proximate cause of Epidemic Cholera.

Thirteen years have elapsed since Cholera commenced its ravages, and a single experiment cannot be adduced to countenance the existence of the peculiar non-electrical state of the atmosphere. Holding, therefore, his opinion to be perfectly gratuitous, we shall leave Mr. Annesley to the enjoyment of the more solid honors which have been awarded him in the practical department.

Several theoretical writers agree with the practical, in supposing that immediate and increasing diminution of the nervous energy, is the proximate cause of the malady. Dr. Good* does not express his sentiments fully and explicitly upon the subject, but from the little that he says, and as he leaves the opinion uncontroverted, it may be presumed that he was inclined to the same conclusion. Of the remote cause, Dr. Good offers no solution.

To dissent from highly respectable authorities, may be considered in us an act of temerity, both on account of the difficulties appertaining to the question itself, as well as the numerical danger incurred in opposing, to the opinion of the many, that of an individual. Appealing, however, to facts admitted by all, and elucidating these through the light of a clear analogy, we hope to be able to show that, during the first stage of Spasmodic Cholera, the nervous energy is not in a state approaching to exhaustion; but, on the contrary, that *it is locally in excess, and to the influence of this excess upon certain parts of the body should the phenomena be ascribed, which externally mark the disease.*

The prevalent opinion, that debility is the immediate cause, probably originated in the medical observers drawing their conclusions erroneously from the effects of the symptoms of Cholera, as had previously been done by others with regard to

* Study of Medicine, by John Mason Good, M. D. 1829.

fever. But it is evident, that in quest of the fountain we should not follow the river to the sea. In whatever stage of the disease the investigation be commenced, we must take the symptoms individually, and ascertain their relations and priority of origin. Guided in this way by the land-marks of observation, we shall finally arrive at a knowledge of the first sensible indication of the morbid action which had been excited within the body, and the next step will be to determine, with the assistance of experience and analogy, the nature of the insensible or hidden derangement, which constitutes the proximate cause.

If general nervous debility be the cause of the symptoms, we would ask how it comes to pass that the mind of patients labouring under Spasmodic Cholera, remains perfectly clear and collected to the last, after the pulse had ceased to vibrate in the limbs, and when death has crossed the threshold of existence? In typhous fever it is otherwise. Here the decline of the cerebral function is evinced in low muttering delirium; in the loss or perversion of the external senses. The ear is mocked by the imaginary fall of waters; the eye deceived by the unsubstantial creations of the brain. The medical treatment, also, found most efficacious in opposing the onset and progress of Cholera, demonstrates that general nervous debility cannot be the cause of the earlier symptoms. When a patient is first seized, a copious bleeding from the arm, and a large dose of laudanum, are the remedies, chiefly to be depended upon, and these, if resorted to on the approach of the malady, will be generally successful in checking or moderating its progress.

It may be alleged, "the principle of this curative process is decidedly stimulant; the abstraction of blood instead of depressing the powers of life, tends to strengthen them, breaking the chain of diseased action, and relieving the heart, already oppressed, of a portion of its circulating load; brandy, laudanum, and other narcotics," it may be continued, "are stimulants in the first instance, their narcotic influence being a secondary effect, and therefore they are often successful in counteracting debility in diseases, of which nervous prostration is,

doubtless, the most prominent feature." In reply to these arguments, we can perceive no parallel existing between the treatment suited to a disease of strict nervous debility, such as typhous fever, and that of Spasmodic Cholera. In the cases of the former, where the putrid tendency is developed with the accession, to resort to bloodletting will diminish the chances of recovery to almost nothing. The copious and repeated administration also of narcotic medicines, that prove our sheet-anchor in withstanding the inroads of Cholera, would rapidly extinguish the flickering ray that sheds vitality in typhus.

That local excess of nervous energy is the cause of the symptoms, is supported by the pathology of Cholera. The localities in which this excess is generated, appear primarily to be the nerves connected with the liver, stomach and bowels. The nerves of the limbs, and of the other parts, subjected to spasm, are probably secondarily affected. The effects correspond to the cause. There is uneasiness at the pit of the stomach, and the patient's face wears the anxious and shrunk expression common to severe abdominal distempers. The canal, along which, in health, the bile freely travels to the intestines, is closed by the constriction of cramp. The spasms soon extend to the stomach and bowels, and they, in consequence, are compelled to evacuate their contents. The patient now complains, for the first time, of extreme exhaustion. This is worthy of special remark as corroborative of our views, that debility is an effect, and not the cause, of the earlier symptoms.

The character of the attendant spasms would not lead us to suspect a diminution of energy in the nerves distributed to the muscles affected. They are of the rigid kind. Is inordinate and continued action, then, the result of debility? Does deficiency of stimuli excite muscles to extraordinary contractility? Surely not. That the patient will be reduced to a state of helpless exhaustion, is no reason against entertaining the previous redundance of nervous influence. The concord of the animal functions, voluntary and involuntary, which characterizes health, is broken by the undue accumulation of energy in any one of the textures subservient to them. Nor do the instan-

ces in which Cholera proves almost instantaneously fatal, militate against our position. They only show that a stimulus, which in a common degree of activity excited undue action alone, will in a greater measure completely destroy it. Thus electricity, in graduated shocks, may recruit, or derange, or destroy the body. In the same way, cold, under ordinary circumstances, is a bracing stimulant, but in excess it is dangerous to life. Mr. Scott has recorded a curious case, which, in so far as the nerves of the extremities are concerned, bears hard upon the doctrine of debility. It is that of a man who had been subject to paralysis and total numbness of his limbs. In addition, he had the misfortune to be seized with Spasmodic Cholera, when, to the surprise of his attendants, his limbs became the seat of spasms, and also exquisitely sensible.

The possibility of effecting a cure in Spasmodic Cholera, greatly depends upon the time in which the patient is submitted to medical management. Should the disease be allowed to completely develope itself before advice is obtained, it will frequently baffle the exertions of the most skilful practitioner, and prove rapidly fatal. But if the physician be consulted when the symptoms are moderate, when uneasiness and anxiety are chiefly complained of, after the use of the customary remedies, strong hopes of recovery may be indulged. Bloodletting, and a large dose of calomel, should be immediately prescribed. To these should succeed constant frictions of hot flannel to the skin; internal prescriptions of laudanum, brandy and water, and other sedative anti-spasmodics, to be used at such intervals of time, and in such quantities as the physician may consider best fitted to the peculiarities and urgency of the case. The signs of returning health will be recognized in the re-appearance of bile in the evacuations. The secretions of saliva and of urine, which had been suspended during the severity of the attack, will be again restored. The breath and skin will gradually recover their natural heat; and, in short, every function of the system will return to the salutary standard.

Should the patient delay application for advice until the

disease has advanced considerably into the second stage, venesection will generally prove useless or injurious. The cramps are established, and they should be alleviated by friction, and the exhibition of anti-spasmodic medicines. If the third or last stage have set in, a discriminating judgment must also be exercised. As debility has now become our only opponent, the sedative preparations are to be modified accordingly, so as to produce little more than a stimulant effect; for the use, at this period, of laudanum, &c., in quantities suited to the treatment of spasms, would render the catastrophe inevitable.

Although we are unacquainted with the individual remote cause that imparted to Spasmodic Cholera the epidemic and fatal character which it assumed in the town of Jessore during 1817, it is clear that it must have depended upon one of the three following circumstances:—1st. Either some peculiarity in the condition of the atmosphere; 2nd. Or some peculiarity in the condition of the human body; 3rd. Or upon a peculiar condition of both. As the various writings upon the subject do not rise above speculation, we shall pass them over to inquire in what manner the disease is propagated in the present day; whether it is communicated from one person to another by contagion? Or whether the atmosphere is the sole remote agent in exciting the distemper?

A few years ago Dr. Maclean divided the medical world into contagionists and non-contagionists. Our sentiments are decidedly opposed to those of Dr. Maclean; yet, notwithstanding, we suspect that the attention he received at the hands of the College of Physicians, was not commensurate to his abilities. The public, moreover, through sheer ignorance of the steps leading indirectly to the temple of science, whilst it swallows with avidity the monsters of quackery *practice*, is ever ready to raise an idle clamor against theories—medical theories in particular.

> To doubt the sun's a sea-coal fire,
> Would mightily displease
> Some folk, who think the whey-faced moon
> Is made of recent cheese.

"In the world of wisdom theories abound." Prosecuted by

men of abilities, even false theories are often productive of much good. The hasty growth of the structure, lacking bone and muscle, either destroys itself, making men wiser by experience; or it demonstrates how far the theory, which has entered upon the right road, may be received. Dr. Maclean failed to prove the non-contagious nature of Egyptian plague, but the College of Physicians were obliged to admit that some modification, advantageous to the interests of commerce, might be made in the quarantine laws.

The fundamental error which governed Dr. Maclean, was an extravagant love of uniformity. He forgot that Nature, while she carefully preserves the family likeness, frequently leaves details to the "beauty of contrast." His first grand position* was, that fevers truly contagious could not affect the same person more than once in a natural life. Therefore, Egyptian plague and typhous fever were non-contagous. Secondly, that epidemics are not propagated by contagion, but depend upon atmospheric causes. Unmindful that he was himself the maker of these "laws," he called them fixed and unchangeable.

The distinction drawn between epidemic and contagious diseases, was altogether fanciful. The fact is, that contagious diseases may become epidemic; and epidemic diseases, originally dependent upon atmospheric causes, may become contagious. Scattered cases, for instance, of small-pox—a disease, the contagion of which is unquestioned—are constantly occurring in various parts of England; but occasionally it attacks great numbers of children, about the same time and place, or, in other words, it assumes the epidemic form. The contagious fever of measles is obedient to similar laws.

Contagious diseases are communicated from one person to another in two ways, either through the medium of contact, or by close exposure to exhalations emanating from a person infected. To avoid a war of words, these affections may be said to have an *animal origin*. Diseases, on the other hand,

* Dr. Maclean on Epidemic and Pestilential Diseases, 1817. Ditto on the Evils of Quarantine Laws, 1824.

that spread independent of contact or animal exhalations, may be said to have an *atmospheric origin*. The task of arranging these maladies under two distinct heads, in relation to their causes, appears to the uninitiated a work of extreme simplicity. But it is not so; for, as we stated above, they do not always retain the identical character that distinguished their first appearance. Several fevers vary in their cause. The same disease may at one time be referred to the atmospheric, at another to the animal origin.

Egyptian plague furnishes an example of the variable remote cause to which we allude. This disease prevails during the winter half of the year in Lower Egypt. It is highly contagious; but though communicated rapidly from individual to individual by exposure to pestiferous contact or exhalation, there is sufficient evidence to show that persons resident there may contract the disease without having undergone any such exposure, and that afterwards they may, by contagion, transmit it to others. Now, a physician who had witnessed a few of the spontaneous cases, if we may so call them, judging from his own limited experience, might ascribe every instance of the disease to an atmospheric origin exclusively; while another, who had only met with instances the result of contagion, might ascribe every case as exclusively, and with equal justice, to an animal origin. This view of the matter may diminish the astonishment with which the public behold the discrepancies of medical evidence.

The admission of a variable remote cause for some contagious diseases, is forced upon us at home in the history of typhous fever. It is commonly propagated by a specific contagion; yet certain external circumstances are sufficient of themselves to generate the disease in the human body. These are damp houses, ill-ventilated, and crowded with inhabitants; wet winters, scarcity of food, and all the pressing concomitants of poverty. In this way have originated hospital, camp, and jail fevers, which are of a contagious nature, and may be collectively included under the appropriate name of *typhus*.

The digression upon the animal and the atmospheric origin

of Egyptian plague and typhous fever, was introduced to prepare the reader for a few observations regarding the origin of Spasmodic Cholera. While the disease was restricted to Hindostan and its neighbourhood, the members of the medical profession in India endeavoured to discover the medium through which it was propagated. A large majority more particularly of the Bengal Presidency, declared the disease to be non-contagious. But in Bombay, the contrary conclusion was ably maintained. In justice to the early advocates of non-contagion, it should be observed that the question, at the period to which we refer, wore a somewhat different aspect from that which it assumed under the impression of later events. The progress of cholera had then scarcely exceeded the boundaries of Hindostan, and here the mild and malignant varieties were indigenous. The non-contagionists might, therefore, reasonably ask, "why a disease which began at Jessore independent of contagion, should not likewise be generated in other localities under the influence of atmospheric causes?" We believe that it was so generated in many instances, and upon the strength of this admission we would reconcile several apparently conflicting statements; the leading features, however, developed in the history of the malady, cannot be satisfactorily explained, in the absence of contagion, on any known accidental condition of health and of the atmosphere. These features are—

1st.—Epidemic Cholera* has travelled as often against, as with the course of the winds. In the very face of a strong S. W. wind which blew in that direction for some months, it passed from Bengal to the Deccan. It has prevailed in every kind of weather common to the climates affected. In the driest weather, and during the deluge of periodical rains; in storms, and in calms; under the scorching sun of Arabia, and amid the snows of Russia.

Opposed as are these facts to the usual progress of maladies, the extension of which depend solely upon the atmosphere.

* *Vide* the different Reports compiled by order of the East India Company; and also the publications of individuals upon the subject.

the character of the succeeding, favours, in a still greater degree, the existence of a contagious power.

2nd.—Epidemic Cholera has in general rigidly followed the great highways of human intercourse. Pursuing the line of navigable waters, and the route of caravans, it entered or traversed the different countries. Through India it extended along the rivers Ganges, Hoogly, Jumna, and Nerbudda. Arabia, Persia, and Syria, were penetrated by the Persian Gulf, the Tigris, and the Euphrates. Moscow received the disease by the route of the Volga. China, other parts of Eastern Asia, and the various islands, were infected over sea, as appears from the Cholera making its earliest ravages in the port towns and maritime districts. Agreeing with the disposition of contagious diseases, the Cholera has been most virulent wherever human beings were numerous and concentrated. In densely-peopled cities; in armies encamped, or upon the march; in localities unfavourable to free ventilation—as low sheltered grounds, narrow streets, close dirty houses. The slow rate of progression at which the epidemic advanced from place to place in succession, and the temporary halts which it occasionally made, perfectly agree with a contagious origin; but they cannot be reconciled to an atmospheric. It travels on an average, at a rate varying between ten and eighteen miles a day. But often, in particular instances, much less. Within the Zillah of Nellore it proceeded thirty-two miles in twelve days; in the next twenty-seven days, eighty miles.

Writers who deny the contagious nature of Cholera, rest their belief chiefly upon the circumstance that many persons were attacked without having had previous intercourse with the sick. This objection brings but little weight with it. In Hindostan such cases may have, at times, arisen from external causes, as at Jessore; but in other countries, where Spasmodic Cholera had never been known until the Indian invasion, we would refer them to contagion, for it is notorious that contagious exhalations may be carried about in merchandise, clothing, &c., their infecting energy remaining for a considerable period unimpaired.

The Russian government was of opinion that the Cholera, during 1829, had entered the province of Orenburg with the caravans trading between Orenburg and Boukhara, the commercial entrepot of central Asia. The Russians, indeed, have uniformly treated the disease as if contagious. The medical council of Petersburgh issued quarantine orders, under which every patient was to be strictly prohibited from holding close communion with persons in health. Even the emperor Nicholas, who to encourage the inhabitants, visited Moscow during the prevalence of the Cholera, underwent, before his return to Petersburgh, the usual ordeal of purification in quarantine. How far these precautions are productive of benefit, it is difficult to say. The disease was equally mortal in Russia as elsewhere, comparing the number of deaths with the number of the diseased; but it is a remarkable fact that fewer of the people by far were attacked there than in southern countries. Whether this partial immunity resulted from the influence of climate, and the stronger constitutions of the Russians, or the rigid quarantine, or from a combination of these three circumstances, it is, perhaps, impossible at present to decide. In the island of Bourbon, however, where sanatory regulations were prescribed and enforced, the malady spread less extensively than in the neighbouring island of Mauritius, in which these things were neglected. As the character of the inhabitants and of the climate is similar in both islands, this fact is in favor of the utility of quarantine; but the strongest evidence in support of contagion, and the propriety of enforcing quarantine, is, that the appearance of the disease in one country or district, has been generally shown to have soon succeeded to the arrival of persons from another, in which the epidemic had prevailed. In Persia, the gates of Ispahan were closed against the suspected caravan. It consequently passed through Yezd. Shortly afterwards the Cholera destroyed 7000 of the inhabitants of Yezd, while the former city escaped.

Cholera is capricious in the selection of its victims. The infirm and debilitated are its favorite subjects. Yet the best state of health will not ensure exemption. This is not oppos-

ed to our view of the proximate cause; debility renders the system more susceptible of morbid impressions,—be they sedative or be they stimulant. The black population suffers in a greater proportion than the white. It is calculated that four millions of the natives of India have been swept away by the scourge since 1817. A share of the mortality, however, may be fairly attributed to partial or total want, in a multitude of instances, of medical assistance. In one district, the population of which is about 200,000 souls, the cases of Cholera amounted to 15,945; of these, 1294 have been without medicine or medical aid, and there is reason to believe that of the number every individual perished. When proper remedial means could be supplied at an early period, and their use continued, the result was gratifying to the friends of humanity, and creditable to the profession, if the intractable nature of the malady be taken into account. The Madras army consisted of 33,396 men, European and Native. During 1818, and the four succeeding years, there died 5 1-3 per cent. of the whole force; or 23 3-4 per cent. of those who had been attacked by the epidemic.

The laws of Cholera bear the impress of that presiding intelligence who has described a circle, beyond which every species of physical evil must cease to mar the harmony of life. Were the disease to continue its ravages in the same place uninterruptedly for a series of years, it would depopulate the world. Few localities have suffered longer than four or six weeks at a time, under the worst form of the distemper, and to this succeed long intervals of safety more or less complete. It does not, moreover, promise to be a plague that will descend a miserable inheritance to many generations. Some countries, formerly afflicted, are even now returning thanks for permanent relief; and in most, the destroyer has relaxed its severity. Wherever it may next direct its course, the principal danger is to be apprehended. May its footsteps be averted from the dwellings of the poor in Great Britain and Ireland!*

* To elucidate the subject of Cholera still farther, a chart of the principal towns and countries, traversed in its geographical progress, accompanies this paper in the Englishman's Magazine. We regret not being able to present it to our readers.

ART. IV.—*A letter from* SIR ANTHONY CARLISLE, *Bart. to the Lord High Chancellor of England, on Cholera Morbus; with remarks by* CHARLES CALDWELL, M. D.

MY LORD BROUGHAM.—It being reported that his Majesty's Ministers have thought fit to take the advice of the College of Physicians, concerning the threatened spreading of cholera morbus, and recollecting some unsatisfactory official medical opinions on former occasions, I have thought it possible that your Lordship's penetrating mind would be better enabled to direct practical measures respecting this important subject, after a review of several statements.

My pretensions to your Lordship's notice are those of a public man possessing extended experience, and whose professional cogitations are founded on a desire to unite scientific precision to the complicated circumstantial evidences from the common fluctuating theories of the healing art.

I consider the long continued verbose medical disputes about infection, contagion, epidemics, and endemics, to be the unprofitable jargon of schools, unworthy the notice of men of science, and as mystified impediments to students, and as being of no use among sensible practitioners.

Abundant evidence to be found among the conflicting doctrines of medical writers, and the narratives of travellers, show that the very same disease may, under different circumstances, assume the various characters of being contagious in one place or climate, and not so in another; similar differences and vicissitudes also occur in the technical distinctions about infections, epidemics, and endemics. The just and broad view of these apparently opposing evidences is, that they are reconcileable under a rational admission that the communicable diseases in question admit of variable degrees of intensity, so as to render them more or less apt to be propagated. Happily for mankind, the morbific materials of these diseases are required to be in a definitive quantity to produce their respective maladies, and the composition of all these morbific poisons seems to be of tran-

cient duration. Hence, by continued dilution, or by a gradual abatement of their acrimony, these pestiferous ferments become inert, and eventually their corresponding diseases cease. This appears to be the best reasoning for the establishment of the laws and regulations of quarantine, and experience has sufficiently proved that a rigorous and continued purification of suspected persons and goods is the only means for preventing the progress of cholera morbus. Should, however, this direful visitation afflict our country, it cannot be looked upon as equally fatal with the worst kinds of pestilence; and its prevention, as well as its curative treatment, promise to be more within medical control.

In addition to the strictest cautions of quarantine, it may be well to ascertain the states of human health which render individuals more or less liable to have this disease, and whether any system of dietetic regimen tends to favor or avert its attack, or to render the event more propitious. From personal interviews with many medical and other gentlemen who have undergone this dangerous ordeal, I am convinced that inordinate doses of calomel given on the presumption of disordered liver, and also an equally violent and mischievous practice of bloodletting, have added greatly to the number of deaths. Both these hypotheses appear to be conjectural, and the fatal effects of the practices justify a public prohibition.

The usual violence at the commencement, and the sudden prostration of the vital powers in cholera, seem to depend on two sensible causes—the first, a manifest abstraction of fluid from the volume of circulating blood through the excessive alimentary discharges; the second a notorious change in the qualities of the blood itself, whereby its natural colour and texture, which are essential to health, are strikingly deteriorated; and since the ordinary stimulants of ardent spirits and wine have either failed to uphold the sinking frame, or have seemed to be hurtful, the employment of ammonia and opium have been adopted with better success. But a suggestion communicated to me by Dr. Ainslie, the learned and laborious author of a valuable book entitled *Materia Indica*, deserves immediate

and serious attention; and as the Doctor has sent his proposal to the Russian government some weeks ago, he has a right to precedence in this important matter, if happily his method should prove effectual. The Doctor viewing the sudden sinking of strength, and the disordered state of the blood, as the most urgent symptoms, advises the immediate trial of inhaling a super-oxygenated air, for the purpose of revivifying the blood, until other remedies, such as ammonia, shall have time to act upon the morbid materials in the alimentary passages. I am more disposed to place reliance upon the immediate and often repeated doses of ammonia, because of the attending spasms in the limbs, an occurrence which is generally connected with alimentary acidity. So that in addition to the cordial effects of ammonia, it may probably act as an antidote. Craving your Lordship's pardon for this hurried address, and the mode adopted for its publication, I have the honor to be your obedient servant, ANTHONY CARLISLE.

Langham-place, June 15.

Remarks.—To detect error and guard against its influence, especially if it be of long standing, and supported by high authority, is tantamount, in some cases, to the discovery of truth. It is chiefly on the ground of this belief, that we have deemed it advisable to publish the foregoing letter, and to submit to our readers the following remarks on it.

High as is the professional standing of Sir Anthony Carlisle, and confident as he appears to be in the validity of his opinions, his communication to the Lord High Chancellor of England compels us to believe, that he possesses but a very limitted knowledge of the true philosophy of either *cholera morbus*, as that disease now prevails in Asia and Europe, or of any other epidemic complaint. While condemning what he justly denominates the "unprofitable jargon of schools," it is his own misfortune to become entangled in some of the very worst kinds of it. We must not pass, without notice, our author's allusion to the contagious nature of cholera. It is thus expressed.

"Abundant evidence to be found among the conflicting doc-

trines of medical writers, and the narratives of travellers, shows that the very same disease may, under different circumstances, assume the various characters of being contagious in one place or climate, and not so in another."

This is a mistake. *Circumstantial* contagiousness is a phrase without meaning, adopted as a mere show of sagacity, or as a cover for a want of knowledge, or a crudeness of thought. A disease must be contagious *in its nature*, else no "circumstances" of "place or climate" or season, can render it so. And if such be its "nature," contagiousness adheres to it, *as a part of itself*, and cannot be separated from it. To prove contagious, a complaint must be productive of a *secreted poison, specific in its character*, and is, therefore, as radically different from a complaint, in which no poison is secreted, as any one species of plant or animal is from another. Convert a disease, which does not secrete a poison into one that does, and you change its species as completely as you would change the species of a *poisonous* animal or plant, by converting it into one devoid of poison—as completely as you would change the fell rattle-snake, by converting it into the harmless black-snake, or the deadly aconite, by transforming it into asparagus. Let the experiment be tried, and the issue correctly noticed. Small pox is converted into gout, intermitting fever, or some other non-contagious complaint. It is small pox no longer, one of whose elements is to be contagious *under all circumstances*, whether of time, place, or season. Again; scrophula is changed into small pox. It is no longer scrophula, one essential quality of which is, that it is *never* contagious. Place, climate, and season, then, may *modify* disease; but they cannot *revolutionize it*. They can render cholera morbus more malignant and fatal, in one instance, and less so, in another. But they cannot render it at one time contagious, and at another not. The advocates of the contagiousness of yellow fever, when driven from one position to another, until their cause became hopeless, took shelter, at last, as their final retreat, behind the assertion, that that disease is contagious *under certain circumstances*. But those circumstances they could not designate. The reason is plain.

They had no being. Hence the hypothesis is now universally rejected, by all physicians, whose experience has rendered them competent judges. And the same fate awaits the hypothesis which affirms the *circumstantial contagiousness* of either cholera morbus or any other disease. A complaint really contagious requires no "circumstances" to render it so; and no "circumstances" can impart contagion to a complaint devoid of it. Every fact connected with its history shows, that cholera, as it now prevails in the Old world, is no more contagious, than it is in the New, when produced by a mess of cherries, currants, or unripe apples. And the issue will prove, that no quarantine arrangements can arrest its progress. Should America escape its ravages, she will owe her safety to the Atlantic ocean, not to any human precautions. Quarantine establishments, when directed against *epidemic* complaints (and such is the present cholera of the east) are founded in error, not to say ignorance, have never proved useful, and never can. The assertion that certain cities have escaped cholera, by prohibiting intercourse with places where it prevailed, is gratuitous. But more on this topic presently.

Equally unwarranted is Sir Anthony's declaration, that the "morbific materials" of cholera and certain other diseases, "are required to be in a *definitive* quantity to produce their respective maladies." On this point nothing is *definitively* known. All that can be said respecting it is conjectural. Small-pox is the only febrile complaint, of whose poison we have any *certain knowledge*. And there is reason to believe, that the smallest possible quantity of that poison produces the disease, as certainly as the largest. Inoculation proves this. Insert under the cuticle the fiftieth part of a grain of the virus, and you generate the complaint. Insert a hundred times as much, and you do no more. Of the quantity of poison necessary to produce cholera, we know nothing. To speculate about it therefore is useless, not to say idle. No doubt, much depends, as to its susceptibility of infection, on the condition of the body exposed to the poison.

No less unfortunate is our author in asserting, that "experi-

ence has sufficiently proved, that a vigorous and continued purification of suspected persons and goods is the only means for preventing the progress of cholera morbus." No proof to this effect exists. On the contrary, every "means" hitherto employed "for preventing the progress" of that pestilence, which yet "walks in *darkness*" as profound as that which surrounded the pestilence of Egypt, *has failed*. The statements of the most enlightened writers on it are conclusive on this point. They represent the disease as neither contagious in its nature, nor to be resisted in its march by any barriers that man can erect. The reason is plain. It has possession of the atmosphere, which human means are unable to control. Nothing but the laws of nature, operating on an extensive scale, can so far change the aerial ocean, in which we live, as either to produce an epidemic disease or stay its progress. The efforts of man to that effect would be as futile and fruitless, as they would be, in an attempt to tranquillize the ocean of water, when lashed by a tempest.

But perhaps Sir Anthony's most striking mistake remains to be mentioned. It is to be found in his opinion of the pathology of Cholera. The following is the hypothesis to which we allude.

"The usual violence at the commencement and the sudden prostration of the vital powers in cholera, seem to depend on two sensible causes—the first, a manifest abstraction of fluid from the volume of circulating blood through the excessive alimentary discharges; the second, a notorious change in the qualities of the blood itself, whereby its natural colour and texture, which are essential to health, are strikingly deteriorated."

This is a very superficial and unscientific view of the subject, to which it relates, and shows our author to be a miserable pathologist. It is gross humoralism, and would have been well received in the seventeenth century, but is obsolete in the nineteenth. "The sudden prostration of the vital powers in cholera" is not owing to either the "abstraction of fluid from the circulating blood" by "excessive alimentary discharg-

es," or to any deterioration "in the qualities of the blood." These are nothing but symptoms of the complaint, and must not therefore be elevated to the rank of primitive causes. The source of the deep prostration experienced in cholera is the deleterious impression made on the nervous and cerebral system, by the atmospherical poison which produces the complaint. In evidence of the truth of this, it is sufficient to state, that the most suddenly and certainly fatal cases of cholera, where prostration and debility are deepest and most distressing, are those, in which the "alimentary discharges" are not very "excessive." In many instances the sick suddenly fall, and expire, in a very short time, (some writers say in "ten minutes") with but limitted discharges. Copious alimentary discharges, of a proper character, are a favourable symptom, and contribute to preserve life, not to destroy it. A leading object in the treatment of the disease, is to excite them. Besides, plague and yellow fever prove, at times, as deeply prostrating, and as suddenly fatal, as cholera morbus, without any alimentary discharges at all. The reason is the same; the deleterious effect (we might say shock) produced by the poison on the nerves and brain. The deterioration of the "qualities of the blood" is also a subordinate symptom, not a primary cause. It arises immediately from two sources, the weakened action of the heart, and the diminished and perverted function of the lungs. These again result from the injury sustained by the nerves and brain, which superintend the other organs of the body, and are chiefly instrumental in giving them vigour.

It is our author's erroneous view of the pathology of cholera, that has led him to approve of the inhalation of "super-oxygenated air," a remedy proposed by Dr. Ainslie, "for the purpose of revivifying the blood." It is puerile thus to talk of "revivifying the blood," without first revivifying the lungs and heart; and they must derive their revivification from the removal of the morbid condition of the brain and nerves. The lungs form the blood, by their specific function, as the stomach and intestines form the chyle, by means of theirs. As soon,

therefore, shall the human body be nourished and strengthened, by filling a paralyzed stomach with food, as the blood be improved, by filling the lungs with "super-oxygenated air," when they are so far deranged as to be incapable of acting on it. A belief the contrary of this is the result of what is denominated "chemical physiology," one of the emptiest hypotheses, that has ever made its way into medical science. Let the lungs and heart be reinvigorated, by a restoration of the cerebral system to soundness, and the common atmospherical air is all that is required to revivify the blood. Without such reinvigoration, no air that can be employed, produces any salutary effect on it. Dr. Ainslie's remedy, therefore, will prove nugatory. Of Sir Anthony's desire to prohibit, by public authority, any mode of practice, instituted and pursued by enlightened physicians, we shall only say, that it exhibits more of a domineering temper, than of correct reason or sound judgment. We should see, with much regret, a public interdiction even of his inhalation of super-oxygenated air. Yet nothing could be less beneficial than such a measure. If it did not actually kill the sick it would suffer them to die, for want of suitable aid. Still, professional exertions ought to be free.

Is any one inclined to inquire what our own views are of the nature of cholera morbus, now raging in Europe and Asia, the means of checking its progress, and the best mode of treating it? They have been already in part communicated. A few further remarks will complete the developement of them.

We believe the complaint to be, not contagious, but a true epidemic, subject to the laws which govern other diseases of the same class.

Like all other epidemics, it travels much more rapidly than any complaint simply contagious can do, its march being sometimes from fifteen to eighteen miles a day. Within the last fourteen years, it has overrun a greater extent of the surface of the globe, than the small-pox has ever done in a century.

It banishes from the place where it prevails all other general diseases, or assimilates them to itself. Neither typhus fever, bilious fever, nor even pestis vera can exist, in their com-

mon forms, within its limits. The reason is obvious. It has possesssion of the atmosphere, and is therefore supreme. This is another law of genuine epidemics.

As relates to its existence, it is independent, or nearly so, of the influence of place, climate, and season. It prevails alike in high-lands and low, following, however, occasionally the courses of rivers, and other bodies of water, but, by no means adhering to them constantly. The torrid and the temperate zones have experienced its devastations, with about equal severity. No change in the *sensible* qualities of the atmosphere seem capable of entirely stopping its course. In temperate and high latitudes the cold of winter weakens it, and retards its march, but does not extinguish it. This shows it to be more exclusively an atmospherical disease, and less dependent on the influence of season, than either plague or yellow fever usually is. Its spread is irregular. It does not march from country to country, with an even front, or in an unbroken line. It often leaps from one place to another, passing over a wide interjacent region, without touching it, and afterwards invades it in several remote points at once. Nor does it spread, where it is prevailing, from the sick to the well. Persons in close and constant attendance on the diseased, are no more liable to it, than those who keep aloof. Nor, when the complaint is contracted in a deleterious atmosphere, and the patient is removed to a healthy one, does it spread among those around him. These are undeniable marks of a disease propagated, not by contagion, but by an insalubrious atmosphere. In other words, they betoken a true epidemic.*

*We know that contagionists attempt, with some adroitness, to maintain their hypothesis, by alleging that epidemic diseases, when spreading over countries, appear first in armies, populous cities, and other places of general resort, where human beings are crowded together in large masses. This they attribute to the constant and extensive intercourse kept up between these assembled multitudes and the places where disease prevails.

The fact alleged is admitted, but the inference is not. It is true that epidemic complaints do generally appear first in armies, cities, and other crowded situations. But this is not attributable to the consideration assigned by the contagionists. It is because the inhabitants of large cities are, from the peculiarity of their constitutions, more susceptible of disease, and soldiers, from the

As respects the means of arresting the progress of this scourge of our race, I believe there are none under human control. Man can neither produce it, destroy it, nor set limits to it. Nature alone, in her own way of working, which we understand not, is competent to the task. Quarantine establishments are impotent in relation to it. Erect them in every seaport and other mart of every portion of the globe, and they will be of no avail. Interdict commerce, and every other kind of social intercourse, in the countries which cholera has invaded, and still the disease will continue to spread, until nature shall stay it, by her own interference. And that interference will consist in restoring soundness to a vitiated atmosphere. On the protection of quarantine regulations, in our own country, we place no more reliance, than we would, were they designed to secure us from the visitations of northeast storms. They will embarrass commerce, expend money, and be useless to health. As relates to the usual intention of them,

exposures and hardships they sustain, more liable to it, than those who live scatteringly in country places.

It is known that the constitutions of the inhabitants of cities are more delicate and sensitive, and less vigorous than those of the robust inhabitants of the country. They are therefore less able to resist any deleterious agent that may exist in the atmosphere. The same amount of malign influence which subdues and sickens them, is insufficient to produce a similar effect on their hardier and less susceptible country neighbours. Hence, on the occurrence of an epidemic condition of the atmosphere, the constitutions of the former yield to the evil first, while those of the latter resist more obstinately, and somewhat longer. They sometimes resist until the epidemic influence terminates, and their possessors enjoy an exemption from sickness. And the hardships and privations of soldiers, acting as exciting causes, render them as liable to epidemic diseases, as the inhabitants of cities; in many cases more so.

The fact, that epidemics spread most rapidly, at times, along the low grounds of rivers, and through flat sickly tracts of country, is explicable on similar principles. Owing to their breathing a less salubrious atmosphere, the inhabitants of such places have less vigorous constitutions, and are therefore less able to resist an epidemic influence, than the inhabitants of more elevated and healthy situations.

The present cholera morbus of Europe and Asia, spreads most rapidly, and exhibits the highest degree of malignity, during the summer and autumnal months. The cause of this seems obvious. The heats of the period referred to, united to the malaria resulting from the dissolution of dead organic matter, weaken the human constitution, and prevent it from resisting the epidemic influence.

and their administration, they are founded on a superstition as palpable, and almost as disgraceful, as that which induces many of our fellow beings to nail horse-shoes over their doors, as a charm against witch-craft. Not a single well authenticated fact can be adduced to show, that the poison of cholera can attach itself to either articles of apparel, or merchandise, or to the human body, whether in health or disease, be conveyed from one region to another, and thus propagate the complaint. On this point contagionists might be safely defied.

Having never seen a case of oriental cholera morbus, our remarks on the treatment of the complaint must be brief and general. It is obviously an abdominal disease, marked with deep congestion of the abdominal viscera. The leading curative indications, therefore, would seem to be three; to allay irritation, promote free and healthy abdominal secretion, and determine to the skin by revulsive agents.

The chief means of executing these indications are simple and obvious. They consist in blood-letting, when the condition of the system calls for it, calomel and opium, judiciously administered, mild diluting drinks, and the warm bath, aided, as they may be required, by rubefacients and fomentations, and other remedies, calculated to elicit centrifugal action. In Sir Anthony Caslisle's ammonia we repose no confidence. Should the disease invade France, with the violence and malignity it has elsewhere exhibited, it may be safely predicted, that it will subvert, for the time, the feeble practice of Broussais and his followers. It will convince them that a giant-foe must be combatted with giant-arms.

ART. V.—"*On the Asiatic Cholera as it appeared in Russia in the years* 1829 *and* 1830;" *taken from the Edinburgh Medical and Surgical, Journal*, No. CVIII, (July, 1831,) with some remarks by JOHN E. COOKE, M. D.

[THE remarkable prevalence of Cholera in several parts of Europe and Asia for some years past, has attracted much attention in Europe, and lately in this country; and there seems to be some danger, from the manner in which the disease is mentioned in various publications, that the public will imbibe the idea that it is a contagious disease. That our readers, many of whom may not yet have met with much on the subject, may be somewhat prepared to form an opinion of the disease, the following interesting paper relative to it, from the Ed. Med. and Surg. Journal, is presented to them; which will be followed by some remarks on the opinions therein adverted to. The paper is given entire.]

THROUGHOUT the whole diversified objects of medical science, there is perhaps none of so much interest at the present moment as the Asiatic or Epidemic Cholera. Its extensive diffusion in the course of a few years over a great part of the eastern world, its almost unexampled mortality, the appalling swiftness with which it brings its victim to the grave, and the mystery which continues to hang over the cause of its origin and progress,—have for some time rendered it an object of attraction to every scientific inquirer who confines not his view to what passes within the narrow circle of his immediate observation.

But in recent times it has acquired even higher claims on the attention of European physicians. During the few first years after it attracted notice in the East, its devastations were confined to the British eastern empire and some conterminous states. But afterwards vague rumours reached civilized Europe of its advance westward; at last in the autumn of 1829, it suddenly appeared with violence at the eastern verge of Europe, on the Tartar confines of Russia; a year afterwards it rapidly advanced nearly 800 miles westward and arrived at

Moscow; and now it is well ascertained that it has made its appearance at Warsaw, about 700 miles still farther west. In all this long course, whatever may have been its mode of propagation, it has distinctly shown itself to be comparatively uninfluenced by various circumstances of locality and season, which lessen or extinguish other epidemic as well as endemic pestilences. It is not improbable, then, that the western confines of Europe may ere long be visited in turn; for the prolongation of its course for eight hundred miles more in the direction it has hitherto traversed would bring it almost to the gates of Paris. In the East it has crossed wider seas than divide these islands from the continent of Europe. If it appears among our neighbours, then, we cannot reasonably expect to escape a similar visitation.

We may indeed comfort ourselves with the reflection, that the easier condition of the lower orders, and superior state of general civilization in the western part of Europe will prevent its farther progress. And it is undoubtedly true that for a long time these advantages have protected western Europe from the inroad of epidemic diseases, or greatly limitted their ravages. But there is, we repeat, a mystery in the mode of propagation of cholera which, in whatever way it may eventually be solved, has hitherto allowed the physician only to discover that it is not exactly amenable to the laws which govern the extension of other diseases, either of a miasmatic or of a contagious nature; and at all events we fear that in the documents now before us some reason will be found for doubting whether comfort and civilization furnish a sufficient barrier against its invasion.

These considerations, independently of others of a more purely scientific nature, which it would be useless to specify, render it an object of extreme interest to the profession and public in every European state to observe the progress of the disease, as made known by authentic documents. The official correspondence of our medical officers in India, to which ample justice has been done in this Journal, has made us fully acquainted with its phenomena as it presented itself in that part

of the globe. Since the publication of their correspondence little new intelligence has reached this country on the subject, beyond a few vague rumours of devastations committed by it in the countries bordering on our Indian possessions, and more recently some scattered notices of its prevalence in various parts of Russia in its progress westwards, communicated chiefly through the medium of the Academy of Sciences and Academy of Medicine at Paris. At length, however, an authentic account has been published of its first entrance into Russia, and subsequent diffusion through a part of that empire; and the original official documents have been rendered generally accessible by the activity of *Professor Lichtenstadt* of St. Petersburg, who in the work now in our hands has translated into German all the material parts of the reports. We have taken the earliest opportunity of bringing this interesting treatise before the reader.

The work consists of two parts. The former is a translation, with occasional abridgement, of the whole Russian official reports, forty-two in number, on the cholera as it prevailed in the province of Orenburg in 1829-30,—taken from a publication by the Russian government which appeared in the course of last year. The latter is a similar translation of various documents, official, and private, illustrative of the second and more violent epidemic which appeared in Georgia in 1830, and in a short while traversed the Russian empire as far westward as Moscow. The documents from which the author has collected his materials are obviously inferior in accuracy and minuteness to those published by our Indian government on the same subject. In some respects, however, especially in the statistical data supplied, they are perhaps superior; in every point of view, they are full of interest; and in one particular they are especially worthy of attention, as, in opposition to the sentiments entertained by almost every practitioner who has witnessed the East Indian cholera, they have led the supreme medical board of Russia to espouse unreservedly the doctrine of the contagious origin of the disease, and, whether this opinion be right or wrong, will also lead every European state to

take measures against its introduction, as if it were really of a contagious nature.

For some years after the propagation of cholera to the north-western portion of British India, little authentic intelligence was received of its progress elsewhere. It appears to us unnecessary to trace the scanty data which the casual notices of travellers and others have furnished on this point. The labour would not be repaid by the results.

A fact of some interest, however, has been stated by Mr. Fraser, in his Travels in Persia, relative to its existence in that kingdom. He mentions that so early as the middle of July 1822, it raged with great violence, to his personal knowledge, in Tabreez, the capital of the province of Azerbijan, about N. Lat. 38¼ and E. Long. 48, and distant scarcely 150 miles, from the Georgian frontier of Russia; that it likewise prevailed much in the country around; and that it soon spread over an interval of 200 miles to Reschd or Resht, a port on the southern shore of the Caspian, and the chief city of the Persian province of Ghilan. Among the numerous anomalies presented in the course of the extension of this strange pestilence, it is not perhaps the least remarkable, that it should have so soon spread with rapidity and raged with violence in these districts, and nevertheless did not appear, or at all events prevail to any extent, or advance to any material distance across the adjoining Russian frontier, till eight years afterwards. In one of the Russian reports, indeed, the introductory report of the medical council of St. Petersburg, it is said to have shown itself in previous years at Astrachan, on the north shore of the Caspian, where, as will be seen presently, it broke out with violence in 1830. But the report is extremely brief on this point; and at any rate there seems no doubt that it was not till August 1829, that the disease attracted particular attention in the Russian empire, and this too in a different quarter from the parts first threatened.

So early as August 1823 the supreme medical council, alarmed at the near approach of the cholera, published Instructions for the Local Boards, to enable the physicians in the frontier

governments to recognize, check, and treat the disease. But notwithstanding this precaution, it seems to have been misunderstood for some little time after its first appearance, although it certainly broke out at once with all the characters and virulence of the Indian Cholera.

The first well ascertained case of cholera, according to the official reports, occurred on the 26th August 1829 at Orenburg, the capital of the province of the same name, situated on the Tartar frontier, 400 miles north of the Caspian, and about 1000 miles, in a direction somewhat to the east of north, from Tabreez and Reschd, where it prevailed extensively in 1822. On that day a man was brought into the military hospital, affected with yellow vomiting, diarrhœa, intolerable pain of the belly, sunken features, blueness of the lips, coldness of the extremities, cramps, imperceptible pulse, extreme exhaustion, and excessive anxiety; and notwithstanding vigorous treatment, founded on the notion that the disease was inflammation, this patient died within twelve hours. No other case occurred for a week afterwards, when a woman suddenly died in the town, of suspicious symptoms, which were believed to be of the same nature. In a week more, on the 8th September, a third person, a joiner, died of twelve hours illness. As his case is a very striking one in many respects, and completely dissipates the doubts entertained by some of the identity of the Russian disease with the eastern cholera, we may here give it in detail, as it is related by Dr. Sokolov, the surgeon who attended the man.

"The disease began at two in the morning, with a dreadful purging, which returned every minute. Although the weather was cold and wet, the patient went out of doors to obey the calls of nature, barefooted and undressed, and without any precaution. About five o'clock he was without feeling, quite powerless, and affected with constant cramps. At six I found him again sensible, but with sunken pale-blue cheeks, dimness of the eyes, coldness of the feet and hands, and bedewed with clammy sweat. He was tossing about, and complaining of trembling of the hands, a sense of oppression at the pit of the

stomach, and intolerable thirst. The vomiting, which according to his own account commenced much later than the purging, was at this time less frequent than it had been; but the alvine discharges continued to recur incessantly, and were passed involuntarily. The exhausted, powerless condition of the patient, in particular his completely imperceptible pulse, both at the wrist and over the heart, the stiffness of the limbs, the coldness of the tongue, belly, and præcordia, left me no hope of his recovery. The administration of opium with oil of peppermint and ether checked the vomiting only for a short time; anodyne clysters had no better effect on the diarrhœa; and warm frictions, spirituous drinks, and even the hot bath were resorted to without success to restore the temperature and bring back the pulse. An unsuccessful attempt was in the last place made to draw blood from a vein; and soon afterwards the man expired. Twenty minutes after his last breath, and when the corpse had been already washed and dressed, it was affected all at once with frightful movements. Convulsive motions took place in the hands and feet, like those excited by galvanism, commencing first in a few muscular fibres, especially in the neck and thighs, extending in a vermicular manner, and suddenly producing bending of the head, and agitation and elevation of the feet. These spasms continued with intervals for ten minutes, becoming in the end faint and rare. The same phenomena, though in a less remarkable degree, were observed on another occasion only, but so long as six or seven hours after the termination of the symptoms of the disease."*–P. 115.

On the 9th two other cases occurred, on the 10th two more, and after this the disease became rapidly very prevalent. It was not till the 10th of September that its true nature occurred to the physicians of Orenburg, who had previously considered it inflammation of the intestines. Its subsequent progress will appear from the numerical statements given in different reports of the Orenburg Council of Health. Between the 9th and 25th of September 57 cases occurred, of which 18 were fatal.

* These muscular contractions after death were also occasionally observed in the cholera of the East Indies.

Between the 9th and 30th of the same month the number of cases was 132, and the deaths 22. Down to the 21st of October the total cases were 747. In the city and suburbs the disease had disappeared entirely on the 20th of November; and from first to last 1100 were seized, and 200 died. When it is added that the population amounts to about 11,000 inhabitants, a clear idea will be conveyed of the extent to which the epidemic prevailed.

It was not till the 23d of September that cases began to appear in other parts of the Orenburg government; and the first place in which it was known to exist was the fortress of Rasupna, 60 miles * west of Orenburg. On the 30th it broke out at Berdsk, a small station 12 miles north of Orenburg, and between the 3d and 8th of October it also appeared in various villages and forts to the west and southwest, at a distance of between 12 and 33 miles. It would be tedious to follow the particulars of its course as it continued to spread from this focus. It is sufficient to mention that from Orenburg it extended about 200 miles northward, about the same distance north-west, and about 60 westward; that the district of country thus visited, measures about 240 miles by 200; and that this space was traversed between the 26th of August and the 6th of February; but that the greater part of it was visited before the middle of November. On the 23d of February the disease was every where extinct. In its course it affected 3590 individuals, of whom 865, or nearly a fourth, perished. The proportion which this bears to the population is not stated; but from various reports it would appear that in towns the proportion was generally about the same as in Orenburg.

The symptoms of the Orenburg cholera are well exemplified in the case already quoted from the experience of Dr. Sokolov. It is needless to dwell on this topic, with the view of proving the identity of these symptoms with those remarked by British practitioners in the cholera of the East Indies. All the Russian reports unite in showing that in the generality of cases

* In substituting English for Russian measure, we have taken the Russian werst as equal to two-thirds of a mile.

there was the same excessive evacuation upwards and downwards of a watery turbid fluid, the same collapse of the skin, coldness of the surface, sinking of the pulse, failure of the strength, lividity of the face, spasms of the muscles, sense of pain or pressure in the region of the splanchnic plexus of nerves, extreme despondency, entireness of the mental faculties, and blackness and inspissation of the venous blood; that in Russia as in Hindostan some instances occurred of rapid death, with collapse and spasms, and without vomiting or purging; that in other instances chronic irritation of the bowels continued for a long time after the violence of the disease was broken; and that sometimes too symptoms of cerebral congestion supervened on the violent constitutional disorder which accompanied the intestinal symptoms, and quickly terminated in coma and death when not properly counteracted. The ordinary duration of the disease in fatal cases was from twelve to twenty-four hours. A few individuals were attacked twice.

The Russian reports have added nothing to our knowledge of the morbid appearances already communicated by the medical officers of the East Indies. In fact, this department of the inquiry seems to have been superficially studied by the physicians of Orenburg. Along with the usual signs of internal venous congestion, most of them agree in saying, that marks of inflammation were found in the intestinal canal. Several however state, that traces of inflammation were far from being invariably present; and on the whole, since they do not particularly describe the appearances set down as inflammatory, it is reasonable to conclude that the supposed signs of inflammation were nothing else than the redness and blackness of the mucous coat of the intestines, which are apt to occur whenever the blood is by any cause driven inwardly in unusual proportion.

The treatment pursued in the Orenburg epidemic may be likewise dismissed in a few words. The Medical Council of St. Petersburg have recommended the physicians of the districts where the cholera might appear, to hold in view the practice recommended by the results of British experience in the East:

and accordingly, after the disease was fully recognized, we find the practitioners of Orenburg withdrawing blood from the arm, and then administering scruple doses of calomel, with opium or other antispasmodics, and stimulating the external surface by friction and the application of heat in every variety of form. And it is a pleasing fact, which the reports now before us fully substantiate, that wherever medical assistance was easily procured, for example in towns, the mortality of the disease was in by far the greater number of instances comparatively low; while it was commonly very great in the small villages and other places where it was for the most part impossible to procure medical advice, till the disease was too far advanced to admit of much impression from the employment of remedies.

The difference in the mortality at different places is very striking and well worthy of notice. In the forts of Rasupna, Nischne-ozernaja, and Iletsk, situated within 60 miles west and south-west of Orenburg, of 621 sick only 33 died, or about one-tenth. In four villages situated in the circle of Orenburg, and within 60 miles of the city, of 240 sick 138 died, or above one-half. In two other villages adjoining those now mentioned, and in three others in the circle of Menzelinsk, at the northern border of the unhealthy country, 89 died of 116, being a mortality of nearly three-fourths. The total average for the whole district which suffered was about a fourth; and this appears also to have been nearly the proportional mortality in the larger towns. On the whole, the mortality seems to have been less wherever a garrison was stationed, a fact which is easily accounted for by the superior civilization in the neighbourhood as well as the more immediate access to medical assistance, and the more rigorous enforcement of the municipal regulations which were called for by the emergency of the case. There is a singular circumstance, however, regarding the relative mortality in civil and military life in the city of Orenburg, which is distinctly pointed out by the statistical returns, but which is neither noticed nor accounted for in any of the official reports; and this is that the mortality was much greater

among the soldiery than throughout the inhabitants at large. Of the 11,000 inhabitants 6000 are considered as of the military class; and of these 299 were affected, and 79 died, or more than a fourth. While among the remaining inhabitants 801 were attacked, and only 121 died, or very little more than a seventh.

We come now to the most interesting of the topics embraced by the Reports, the mode in which the Orenburg cholera originated, and subsequently extended itself. This subject, on account of its great importance, deserves to be considered at some length.

The reader is probably well aware that by an overwhelming majority of the British medical officers, who have witnessed epidemic cholera in the East Indies, this disease is considered not to be of a contagious or infectious nature,—that is, not to be communicable from man to man. A few incidents occurred which excited suspicions in the observers that it really might after all possess this property. But scarcely a single person has advocated the doctrine of contagion with any earnestness. It is well, perhaps, that in the East Indies matters should remain in this respect as they are. For undoubtedly many facts were observed there apparently incompatible with all we hitherto know of the phenomena and progress of contagious diseases; and the facts of a contrary nature are so scanty, that in the face of the lamentable effects which the adoption of the doctrine of contagion would have on the native population, it would be rash and unwarrantable to allow them much weight in the decision of the question. The sequel, however, will show that such facts, meagre and scattered as they may seem, ought not to be lost sight of altogether.

Notwithstanding the almost unanimous and earnest recommendation of British practitioners not to consider the cholera contagious, the Russian government appears at an early period to have been alarmed by accounts received of its progress in the interior of Persia; and arriving at the concluson, that its mode of propagation was extremely suspicious, they resolved that the intercourse with the frontier provinces should be sub-

jected to a quarantine of fourteen days for persons, and to the usual restrictions enforced in times of plague in regard to goods. As the rumors of the devastation committed by cholera in central Asia, however, died away, it is probable that the quarantine was imperfectly enforced.

But whether enforced or not, it was found quite impossible to trace the introduction of the disease into Orenburg to importation from the East. After it broke out in Orenburg information was received that it had been prevailing in several parts of eastern Persia, more especially in the province of Khorazan, and likewise in various districts of Bucharia, above all, in Chiva, a city in the province of Kharazm, situated on the Jihon, a stream which falls from the south into the sea of Aral, and where some of the Bucharian caravans assemble previous to crossing the great Steppes of the Kirghis-Kaisaks. Hence, among the physicians and civil governors of the Orenburg government, attempts were made to trace the introduction of the cholera to the caravans which arrive at Orenburg from central Asia about midsummer. The strictest investigation, however, which could be made entirely failed in attaching any probability to this notion. The last caravan which arrived at Orenburg reached that place on the 22d July, thirty-five days before the first case of cholera occurred there; the individuals composing it were all in a good state of health; and in crossing the Steppes, which is accomplished in from 33 to 90 days, they lost only one companion, whose disease could not be ascertained, but who died after being twenty days ill. It is clear, therefore, that the disease could not have been introduced by any of the persons belonging to the caravan. It was scarcely less certain that its introduction could not have been owing to the goods conveyed by them. For at every resting place it is the practice of the merchants to unpack their merchandize, so that the whole individuals of the caravan must have been freely exposed to the effluvia; yet none were taken ill. Besides, the Bucharian merchants and their servants, laughing at the suspicions of the Russians, exposed themselves with perfect readiness in every possible way to the exhalations from their

bales and packages. Neither could it be discovered that any person in Orenburg was attacked by the cholera, who had purchased goods of a suspected nature brought by this caravan. When to these facts it is added, that at Orsk and Troitsk, two other frontier towns, where the eastern caravans likewise arrive during summer, cholera did not appear at all, we conceive it is distinctly made out that importation in this manner was out of the question.

With somewhat greater appearance of reason, the introduction of cholera was supposed by some of the inhabitants of Orenburg to have been accomplished through means of their immediate neighbours to the East, the Kirghis Kaisaks, from whom the government of Orenburg is separated merely by the river Oural. On this point we shall quote the following judicious remarks of Dr. Sokolov.

"Finally," says he, "the introduction of cholera into Orenburg has been ascribed to our neighbours of the Steppes, the Kirghis. Their intercourse with Taschkent, Bucharia and Chiwa is well known; so that if the disease was introduced into any of these districts from Caubul or Khorasan, it must have been communicated to the Kirghis. Their own accounts too, though discordant in many respects, supply clear, convincing proof that cholera has shown itself in some of their hordes on the Ilek and Emba, (the former of which streams joins the Oural from the south-west, a little below Orenburg, while the latter river runs parallel to the Oural at a distance of about eighty miles, and flows like it into the Caspian.) But their constant distrust of us, and their suspicious closeness are a great obstacle to our procuring distinct evidence of the form and extent in which the disease prevailed in this wandering semi-barbarous race.

"Besides it is their constant custom to abandon all those to their fate who are sick or suspected to be ill, (for example of small-pox, measles or inflammatory fever,) and to wander to a distance from any place where such diseases have shown themselves,—which is, in fact, the most effectual means they could take to check their dissemination, and serves equally to lessen

the number of victims, and to withdraw them from observation. Along our whole confines the Kirghis are constantly employed bartering goods with us during the summer and harvest, their principal articles of commerce being sheep, camblet, felt, and skins. But if the Chiwa and Bucharian caravans could not have introduced cholera into Orenburg, because it was actually never introduced into Orsk and Troitsk, how was it possible for the Kirghis to introduce it into Orenburg, without also introducing it into the numerous stations on the frontiers with which they maintain an equally constant intercourse?"—P. 123.

It appears then that the introduction of cholera from the eastward is rendered improbable, and at all events that the surmises on this head never received confirmation from the strictest inquiry which it was possible to institute.

Quitting this subject, however, we must now proceed to consider the more important question, how the disease, when once introduced, propagated itself from place to place. A variety of views have been taken of this question. At present the general opinion in Russia is, that the disease communicates itself from man to man; but this was far from being the general opinion of the Orenburg physicians, who personally witnessed its progress. Others have conceived that it arose from terrestrial miasma—an idea which will be presently seen to be quite untenable. Others again have thought that a superabundance of the usual causes of common cholera, concurring with some peculiar epidemic state of the human constitution, was sufficient to account for its prevalence; and finally, others bewildered by the difficulty of explaining every incident in its history according to any of these views, have resorted to the vague, hypothetical notion of a peculiar electric state of the earth or atmosphere. In what follows we shall not pretend to discuss at length each of these opinions, especially those of a purely hypothetical nature; but it will be requisite to consider fully what grounds exist for considering the disease as originating in contagion or terrestrial miasma.

For some time after the cholera appeared in Orenburg and its vicinity, the physicians there not only did not entertain the

idea that it was contagious, but likewise encountered many facts which appeared to them incompatible with this opinion. But in the subsequent progress of the epidemic, facts of a contrary nature occurred, so that before it terminated many were staggered in their opinion, and a few became decided contagionists. We shall proceed to consider the evidence which the reports supply in favour of its contagious nature, and which, as will presently appear, amounts to presumptive, but by no means very strong proof.

An important fact which appears favourable to the notion of contagion is that the disease broke out in some places very soon after the arrival of persons ill of cholera who had recently left districts where it prevailed. Several instances of this are reported. Thus, the first place where cholera appeared after it broke out at Orenburg, was in the fortress of Razupna, 60 miles to the westward, down the course of the river Oural. Here it commenced on the 19th of September, passing over several forts and villages on the highway, which, nevertheless, we shall presently find, were attacked subsequently; and it is said, that the first person attacked was a vintner's servant on the very day he arrived from Orenburg, and that the disorder seized several of the garrison four days afterwards.—Again, in the fortress of Iletsk, 42 miles to the S. W. of Orenburg, and on the verge of the Kirghis-Kaisak Steppes, the disease did not break out till the 2d of October, and first appeared soon after the arrival of a soldier, and a soldier's wife, who were taken ill on the way from Orenburg, and died on the day after their arrival at Iletsk. Three days after their death three individuals took ill in the garrison, one of whom was the husband of the woman. Another incident of the same nature occurred towards the close of the epidemic at Caramala Gubeewa in the northern part of the Orenburg government. This village had remained healthy till the 14th of January; but about this time a peasant arrived from a village 24 miles distant, where the disease prevailed, and died of it immediately after his arrival; whereupon this man's nearest relations were attacked; and immediately afterwards the disease spread among the inhabitants.

so that in a short time 41 were attacked, and 20 died. Besides these special facts, there are also some general, but rather vague, statements in the Reports, to the effect, that in no instance did a person go from a district where the cholera prevailed to another where it did not, and then take it without the disease soon afterwards attacking many people in the neighbourhood.

A remarkable circumstance, also favourable to the opinion that the cholera was propogated by contagion, was the peculiar irregularity of its course. Diseases which are supposed to depend on peculiar states of the air, and especially cholera, as it prevailed in the East Indies, are usually believed to advance with considerable uniformity in the same directions. And this rule applies to the Orenburg epidemic considered on the great scale, its course being distinctly westward and northward. But there were several important exceptions, which undoubtedly render its progress more like that of a contagious disorder.—Thus we have seen that in arriving at Razupna from Orenburg, numerous forts and villages on the high road were passed over. But in no long time the disease began to retrace its steps along the road eastwards. It was at Razupna on the 19th September. On the 6th October it broke out at Nischnei-Ozornai, 14 miles to the eastward of Razupna; on the 8th October at Tatischtscheva, 14 miles farther east; and on the 29th October at Rischkova, 14 miles farther east still. Then it appeared to return in its original direction to Zubotschistka, a little to the westward of Tatischtscheva, where it broke out on the 9th November. And again it resumed its backward course towards Orenburg, and on the 19th January broke out for the first time at Tschernoretschinsk, a fort situated 14 miles west from that city.—In like manner, proceeding northward, it arrived in the village of Bikkulov, 60 miles from Orenburg on the 28th September, having affected no other place on the road. Then on the 5th October it appeared at Scitovskoi, only 16 miles north of Orenburg, and on the 14th October at the town of Sackmarsk, 16 miles farther north, and consequently 28 miles south of the first village attacked. On the

same day it also appeared at Sarmanaeva, 24 miles north of Bikkulov; and proceeding in a northern, or rather north-westerly direction, it arrived at Bugulma, a considerable town, 160 miles from Orenburg, on the 7th November. In the latter part of its progress, however, it passed over nearly 70 miles of the road, leaving many villages entirely free. But then it appeared again to retrace its steps, breaking out with violence at Tiriss-Usmanova, 30 miles from Bugulma, on the road back to Orenburg, so late as the 5th December, and subsequently attacking several other villages in the vicinity of this place.

Another fact favourable to the views of the contagionists is, that in some places the disease never appeared, although it raged all round, apparently because the inhabitants cut off all intercourse with the surrounding country, except under the restrictions of a rigorous quarantine. Few examples, however, to this effect are stated by the reporters. Indeed the only one worthy of mention is a single instance which occurred when the disease broke out, as already mentioned, at Caramala-Gubeewa. Some Russian peasants living together, scarcely a hundred yards from the village, shut up their hamlet on the first intelligence of the disease having appeared in their vicinity, and enforcing a strict quarantine during the prevalence of the epidemic, entirely escaped its visitation.

But the arguments now stated, when fully investigated and compared with other contradictory incidents, will be found less conclusive than they appear to be on first examination.

As to the communication of the disease from Orenburg to Iletsk and Razupna, and to Caramala from an adjoining village, by means of individuals travelling from a diseased to the healthy district—it is singular that throughout the reports generally, satisfactory information is seldom given on the question, whether the residenters first attacked in the previously healthy places were exposed to the alleged contagion. There is indeed one strong fact of the kind explicitly stated, in regard to the extension of the disease at Iletsk, by the staff-physician, Dr. Schimanski. He says he was able to trace the progress of the disease in the first eight cases thus:—The husband of

the woman from Orenburg took ill three days after her; and about the same time also, two girls who lived in the immediate neighbourhood of the soldier, and who visited him soon after his arrival from Orenburg: The aunt of these girls, who nursed them, was next attacked; and from her it passed to her own two sons. This, however, is the only fact of the kind which is related with any precision. As to the extension of the disease at Caramala, as we have already stated, it is merely observed in general terms, that the first persons seized in the village were the relations of the man who was supposed to have brought it from an infected district. And with respect to the interesting occurrence at Razupna, where several of the garrison took ill four days after the death of a man who was seized immediately on his arrival from Orenburg, it is distinctly mentioned in one of the reports by Dr. Schumov, that, of the individuals who visited this person, not one was attacked; that of those who were attacked not one had been exposed to the contagion supposed to have been introduced by him; and that its extension in the garrison was not prevented by an early and rigorous quarantine.

Next, as to the irregularity of the course pursued by the disease—while this is at variance with the idea of an atmospheric cause, it is equally true that some facts relative to its course, are not easily reconcilcable with the notion of contagion being its only cause. We have seen, for example, that at Tschernoretschinsk, the disease appeared on the 19th of January, after having passed over this place four months before in its course westward. But it is also particularly worthy of notice, that in all the towns and villages within a moderate distance, but one, the disease had completely disappeared before the end of November, and that in the only village where it continued longer, Cardailovka, at a distance of 20 miles, it had ceased on the 8th December, or more than five weeks previously. Now all the facts hitherto collected in favour of the contagious nature of cholera show, that, if contagious at all, the period during which the contagion remains latent in the body is very short. So strongly indeed was this shown to be the

case, that the Russian government, as already mentioned, limited the quarantine to fourteen days for the person. In all the cases where the disease broke out soon after the sickening of a person newly arrived from a diseased district, it appeared among the residenters in less than a week. If the contagion, which, in the majority of instances remained latent in the body four or five days only, could in some instances have remained latent for five or six weeks, the disorder, as the quarantine for persons was limited to fourteen days, ought to have extended itself over a much greater extent of country, and travelled with far greater rapidity. It is not easy then to conceive how the village of Tschernoretschinsk could have received the disease by communication with infected individuals; neither is it stated that any such communication was actually traced; and as to communication by infected goods, it does not appear that the commencement of the disease in any part of the Orenburg government was supposed to have been referable to such a cause.

The last argument in favor of contagion,—the immunity enjoyed by certain small districts in the heart of an infected country, whenever intercouse was cut off with the places where the disease prevailed,—is certainly a fact of much interest and some weight. At the same time so many instances of similar immunity occurred where the fact of free exposure to the supposed contagion could not be called in question; and so many instances also occurred where the disease spread in defiance of the quarantine, that one is tempted to ascribe the escape of the shut up districts, which, after all, were very few in number, to accident, or at least to some other cause than the suspension of human intercourse.

Several examples occur throughout the reports of entire bodies of people remaining exempt from the disease, although exposed to it in circumstances in which escape would seem next to impossible, if it really propagated itself by contact or exhalations from the sick. We shall be content, however, with relating a single instance only, which is the strongest of the kind we have remarked in the reports. It is mentioned by a staff-physician, Dr. Smirnov. During two months while the disease

prevailed at Orenburg, and 299 patients were admitted with cholera into the military hospital, the personal attendants on the sick remained entirely exempt from the disease. They consisted of one hospital assistant, six pupils, as many Baschkir lads, and fourteen hospital servants, in all 27; and their duties were to perform blood lettings, apply leeches, poultices, and frictions, and administer baths and the like, so that they were compelled to be constantly breathing the exhalations from the bodies and clothes of the sick, as well as to touch and handle them. Besides, the same immunity was enjoyed by certain officers and subalterns who lived in this hospital, and were in the sick-wards once a-day, besides being at other times in constant communication with the hospital-assistant and the pupils.—Farther, the washerwomen of the hospital likewise escaped,—a class of individuals, who, it is well known, are extremely apt to suffer from contagious diseases. One only was attacked with cholera; but she washed merely the clothes of the officers, none of whom had been affected before she took ill. Lastly, Dr. Smirnov, and his colleague Dr. Sokolov, were in constant attendance on the patients affected with cholera, but did not suffer. This immunity enjoyed by the medical and personal attendants of the sick in the hospital of Orenburg is exactly conformable with what had been previously observed in the greater number of the hospitals in the East Indies.

It is, we conceive, in vain to object to the preceding fact as evidence against propagation by contagion,—that the contagion of cholera, like other contagions, does not affect every individual exposed to it,—that some other concurring circumstance is requisite to enable it to take effect. This objection is a sufficient explanation in the case of certain persons escaping in a family or congregation of individuals, some of whom suffer. But can it be held valid where a large body of persons, of all ranks in life, and all freely exposed to the alleged contagion for two entire months, escape to a man? Certainly not. The immunity enjoyed by the officers and servants of the military hospital of Orenburg is surely sufficient to prove, that *at*

this period of the epidemic the disease could not propagate itself from the sick to the healthy.

It is obvious, then, that there are insuperable difficulties in the way of the opinion, that the Orenburg cholera of 1829 at its commencement was a contagious disease, or at least, that it propagated itself after the manner of known contagious diseases,—for this is perhaps all that strict induction entitles us to conclude from the facts. And accordingly, few of the medical reporters of the Orenburg government have espoused the doctrine of contagion; and those who have adopted it, did so with hesitation, and not till towards the close of the epidemic, when they were surprised to find, that it was not more obedient to the laws which govern the progress of disorders of a miasmatic origin, than it had previously shown itself to the generally received laws of contagious diseases.

This leads us in the next place to inquire, to what other cause besides contagion the propagation of the Orenburg cholera can be ascribed; and, in particular, whether it can be reasonably ascribed to terrestrial miasma. But it will be no difficult matter, we apprehend, to prove, that, if the circumstances which accompanied its dissemination are at variance with the received laws of contagion, there were also incidents of frequent occurrence throughout its progress, which are still more incompatible with its cause being of the nature of terrestrial miasma.

Orenburg, the city where it first made its appearance, seems, from the description of Dr. Sokolov, to be one of those chosen spots on the globe, which the physician would select for its exemption from all the circumstances of locality that are apt to engender endemic diseases. It is situated in an extensive plain, abounding in undulations to diversify its surface, destitute of lakes and marshes, traversed by clear streams, and not more wooded than is required to beautify the landscape. It is built on the higher bank of a fine river, the Oural, which runs in a defined channel, and presents no bogs or miry meadows at its margin. It is liberally and cheaply supplied with all the

necessaries of life, in particular with grain, fruits, and butcher's meat of all kinds, and the river abounds in fish.

"On account of its position," says one of the reporters, "its regularity and cleanliness, it presents not the slightest facility for the engendering of disease. One could not wish for a more choice site for the maintenance of human health. This is abundantly shown by the small number of endemic diseases to which it is exposed, and their rare appearance, as well as by the great age attained by many of the inhabitants."—P. 99.

The whole territory comprehended in the government of Orenburg is more or less of the same nature; and in particular, the locality of the greater towns appears uniformly well chosen. How any baneful terrestrial emanations could be engendered in such a district it is impossible to imagine.

Another fact still more incompatible with a terrestrial origin is the important circumstance observed for the first time in the Orenburg epidemic,—that its progress was not arrested by the cold of winter. In the East Indies the observation had been already made, that the appearance and propagation of cholera were by no means invariably connected with any particular season or kind of weather. In Russia it has been further proved to prevail in defiance of intense frost. In most parts of the Orenburg government it continued to prevail long after the frosts set in; in many districts it went on with unabated violence when the temperature was far under zero of Fahrenheit's scale; and in some districts it actually made its first appearance when the thermometer was as low as —29° F. Thus in Tiriss-Usmanova, a Mahomedan village above 130 miles north-west from Orenburg, containing about 700 inhabitants, the disease broke out for the first time on the 5th December, when the thermometer was at —29° F.; under the same degree of cold it prevailed till the 23d of the same month; and in this short interval it seized no fewer than 147 individuals, or above a fifth of the population, of whom 37 died, or nearly one-fourth. In like manner in Sterlitamaka, a town of 2300 inhabitants, about 100 miles north of Orenburg, the disease broke out on the 7th November, and in twenty-two days at-

tacked 79 of the inhabitants, and carried off a fourth part of these, although the temperature all the time ranged from —13° to —35° F. It is well known that terrestrial miasmata, so far as we have hitherto been acquainted with their properties, cannot exist under such conditions.

Another particular which is worthy of notice, as being on the whole at variance with the usual properties of terrestrial miasmata, though certainly by no means so important as the facts already mentioned, is, that elevation above the plain where the cholera prevailed did not constitute any protection. In the East Indies it was, we believe, invariably observed that cholera did not proceed up the mountains. In the government of Orenburg, on the other hand, instances occurred of villages being attacked, though situated at a height of 1400 feet above the surrounding plain. The villages of Jemanguleva and Sarmanaeva, north-west from Orenburg, and situated at this elevation, were attacked, the latter on the 14th October, the former on the 1st November; and in Jemanguleva 73 persons were seized in the course of 23 days, of whom 55 died: while in Sarmaneva the number attacked in four weeks was 113, and the deaths 19.

In the face of all these facts, it appears very difficult to trace the origin or progress of the Orenburg cholera to terrestrial emanations. And at all events it is quite impossible even to form a conjecture as to what these emanations may be, where they are produced, or in what circumstances.

Some of the Russian physicians appear inclined to attribute the disease to the ordinary causes of common cholera prevailing to an extraordinary degree, and favoured by some peculiar epidemic state of the human constitution. In the autumn of 1829, it is observed that there was an unusual abundance of fruits, more especially of gourds and water-melons, which are considered, and probably with justice, to be very apt to induce ordinary cholera, and which in the present instance were regarded with so suspicious an eye, that the local magistracy of Orenburg prohibited the sale of them, or the introduction of them from the adjacent country. So far an immediate exciting

cause was perhaps present. The Russian reports also supply some grounds for entertaining the notion that a peculiar epidemic state of the constitution existed throughout the population at large as a predisposing cause. Something of this kind has been supposed by East India practitioners to be at the bottom of the mysterious appearance and progress of epidemic cholera in that country. But all the facts yet collected on the subject in the East constitute no more than a basis from which to proceed with farther inquiries; and they have not received any material additions from the experience of the Russian physicians during the epidemic of Orenburg. The following observations, however, by Dr. Onufriev, physician for the circle of Orenburg, are deserving of notice, as containing the only evidence worth mentioning relative to the point now under consideration.

"During the prevalence of the epidemic, there was scarcely a single inhabitant of the city of Orenburg who had not some symptoms of disordered digestion. One complained of oppression and pain in the breast; another of headach, slight sickness, looseness of the bowels, and the like. These trifling symptoms of disease were usually ascribed to errors in diet. But to me it appears that their cause was a general invasion of the system by cholera, which, however, was prevented from developing itself in its perfect characters, by a regular manner of living, and other circumstances of the kind. This may be considered as proved by the almost universal prevalence of symptoms of disturbed digestion, their originating without any apparent cause, especially in persons newly arrived at Orenburg, and their departing under the usual treatment."—P. 96.

It would be an object of extreme importance to establish, on satisfactory evidence, the existence of such a constitutional *diathesis;* but it appears hasty to assume its existence on such general and vague evidence as is here presented.

On the whole, the result of this sketch of the contents of the Russian documents is, it must be confessed, to leave the medical world still very much in the dark as to the origin and mode of propagation of the Orenburg cholera. The operation of

terrestrial miasmata could no where be traced; the weather before and during its prevalence presented no peculiarity which distinguished the autumn of 1829 from the same season of previous years; the superabundance of the ordinary causes of common cholera is obviously far too feeble an agent for the production of so formidable a disease as epidemic cholera, without the pre-existence of some morbid peculiarity of constitution, which was too indistinctly traced throughout the Orenburg population; and if the doctrine of contagion be adopted, in default of a more plausible explanation, this can only be done under the admission that the disease might also originate differently,—or at all events, that numerous anomalies occurred, which are at variance with the laws of contagion, as laid down from careful observations, in various countries, and in various epidemics of diseases universally acknowledged to be of a contagious nature.

In this state of matters the mind is naturally led to incline to that opinion which appears to be the safest in its practical bearings; and, accordingly, one or two of the Orenburg physicians even at the commencement, and a greater number of them towards the close of the cholera of 1829–30, either espoused the doctrine of contagion unreservedly, or admitted it to be so probable that they were induced to concur in the propriety of inforcing a rigorous quarantine. This opinion, and the resulting practice, gradually became so firmly established, that when the disease again appeared in Russia, in the autumn of 1830, it was almost every where regarded as contagious, and quarantine enforced as in the instance of the plague.

At the end of February 1830, the cholera, as we have already mentioned, became extinct in the Russian dominions. But about the same season in which it originated the year before, it again broke out with increased violence in a different quarter of the empire; and it has continued ever since to spread, with little intermission, westwards, till the beginning of May, when information was received of its arrival in the neighbourhood of Warsaw, having thus travelled about 1600 miles in the course of eight months. The documents and abstracts pub-

lished by Professor Lichtenstadt bring down the account of the epidemic nearly to its cessation in Moscow. But the facts he has collected are much more general in their nature, and many of the documents less authentic, than in the instance of the Orenburg epidemic. Partly on these accounts, but chiefly because he promises to produce ere long a more detailed history of the second epidemic, we shall for the present pass over the second part of his work cursorily.

The epidemic of 1830 first appeared on the Persian frontier of Georgia, towards the end of July. From inquiries subsequently made by the Minister of the Interior, who was despatched in person to the diseased district, to superintend the labours of a Commission of Health, it was ascertained that the disease had appeared in June in the Persian province of Ghilan, on the southern shore of the Caspian, and more especially in various towns, among which, Resched, the port formerly spoken of, is particularly mentioned. From this it extended itself northward, along the western Caspian shore, till it reached Baku, another port, 200 miles from Resched, early in July. On its way thither it likewise struck off in a north-westerly direction along the river Kur, and thus, on the 27th July, arrived at Tiflis, the capital of Georgia, distant 400 miles from Resched. Here it attacked 579 persons in ten days, and of these 237 died. In this city it was not considered contagious. The particulars which led to that opinion are not stated; but "most convincing evidence," says the report, "was received of its not being contagious, so that the population were allowed and recommended to quit the town and scatter themselves among the adjacent hills,—a permission of which, in a few days, two-thirds of the people availed themselves."

Meanwhile, proceeding northwards from Baku along the Caspian, the disease attacked various ports and adjacent towns, and on the 19th July reached Astrachan, the capital of the kingdom of the same name, situated on an island in the principal mouth of the Wolga, about thirty miles from the northern shore of the Caspian and 350 miles from Baku. Here in ten days 1229 persons were seized, and 433, or fully a third, per-

ished. Its subsequent devastations in this city are not reported, and the report stops at a time when 100 persons were attacked daily. From Astrachan it spread in a north-westerly direction along the Wolga to Enotaevsk, Tschernojar, and Zaritzin, the last of which places, above 250 miles from the mouth of the river, it reached on the 4th of August. At Saratov, a considerable town on the Wolga, 200 miles farther north, it appeared on the 6th of August, and raged with great violence; for down to the 7th September, 2367 deaths are recorded. In its course north from Tzaritzin to Saratov, it also struck off at right angles to the Wolga, and attacking the country of the Don Kossacks had, at the 17th September, affected 1792, and killed 1334 of these, or two-thirds; then continuing its branch-course westwards, it passed through the government of Woronesch, then reached Kharcov 350 miles from the Wolga, and subsequently the government of Kiev, 150 miles farther west. But the main stream of the epidemic continuing to follow upwards the channel of the Wolga from Saratov, turned north-eastward with the river, and arrived at Samara, in the government of Simbirsk, and 200 miles north-east from Saratov, on the 27th August. Here in seven days 47 people died of the disease. At the same time, it passed about 140 miles in a northerly direction from Saratov, across the country to Penza, capital of the government of that name, where it arrived on the 17th August, and in fourteen days attacked 1200 of the population, of whom 300 died. Penza is situated near the source of a tributary of the Wolga running *northwards*, consequently in an opposite direction to the main stream of the river, and falling into it at Nischnei-Novgorod, where the main stream has a winding course from west to east, so as to make a great bend before taking its southerly direction. At Nischnei-Novgorod, the cholera commenced about the end of August; down to the 20th September, 800 persons had been seized; and on the 19th October it had affected 1863, of whom 968 died, or fully a half. Being now in the heart of the European dominions of Russia, it seems no longer to have observed its uniformity of direction. On the 9th September it broke out at Kasan, cap-

ital of the government of the same name, 200 miles *down* the Wolga, and east from Nischnei-Novgorod; and here, up to the 17th October, 1957 had been attacked, and 1174 carried off.* At the same time it spread in a north-west direction from Nischnei-Novgorod to Kostroma, 150 miles *up* the river; and in various parts of the government of Kostroma had, down to the 25th October, affected 430 persons, of whom 125 died. From this place it spread north, north-west, and west, into the governments of Vologda, Novgorod, and Tver; where, however, at the date of the publication of the reports on the 14th November, it had either ceased, or never prevailed but to a limitted extent. In a third direction from Nischnei-Novgorod, namely a little to the south of west, it proceeded along a tributary of the Wolga, and then striking off from it arrived at Moscow, about 260 miles from Nischnei-Novgorod, towards the middle of September. Between this period and the 10th of November, 5451 persons had been attacked in Moscow, and 2876, or three-fifths, died.

Such are the leading features in the progress of the Russian cholera of 1830. The general result is, that the disease advanced in a direction a little west of north from the south of the Caspian to Tver and Vologda, not far from the sources of the Wolga, being a distance of at least 1500 miles, in three months and a half. In this long course it appears to have chiefly followed the great lines of communication from country to country, attacking successively the great towns on the highways, or on the banks of navigable streams. Whether or not it diverged from the highways and towns, and assailed the villages or country districts at large, is not stated in the reports now before us. Neither is it mentioned whether it attacked successively every place in its route northward, or, as in India, passed over whole districts in its way from town to town, or, as in Orenburg, first passed them over, and then returned upon them. These are very interesting points to determine in re-

* While we say the disease travelled eastward from Novgorod to Kasan, it is right to add, that it may also be considered as having arrived at the latter place by taking a north-westerly route from Samara.

ference to the question of its mode of propagation; and it is therefore to be hoped that the promised official reports will enable us to settle them.

There are also many other statistical considerations which we hope will not be omitted, more especially as for some years the attention which has been bestowed by the Russian Government on the statistics of the empire must have given it great facilities in conducting such inquiries. The reports and abstracts translated by Professor Lichtenstadt, for example, do not convey any information on the proportion of cases to the population of each place, the relative mortality of different periods of the epidemic, in different ranks of life, in different circumstances of locality and access to medical advice, the relation of the mortality to that of ordinary years from diseases generally, and many other similar points of much interest in a scientific and practical point of view. The following facts of the kind now alluded to we have gleaned throughout the documents presented by the author. The mortality, which was every where considerably greater than in the Orenburg epidemic, differed much in different towns, being in Astrachan one-third of the cases, in the government of Kostroma rather less, in Nischnei-Novgorod one-half, in Kasan and Moscow three-fifths, in Penza and in the country of the Don Cossacks two-thirds,—and in the whole of these places taken collectively, the number affected was 14,000, and the deaths 7700; so that the total average mortality throughout the invaded districts may be safely assumed to have been one-half. The mortality in Moscow varied greatly at different periods of the epidemic, being at first so high as nine-tenths of the cases, and afterwards sinking gradually to seven-eighths, five-sixths, three-fourths, a half, and at last to a third. The decrease was probably owing in part to a change in the virulence of the epidemic; but much must also be ascribed to the zeal of the government in providing the means of subjecting the patients in the poorer ranks to medical treatment at an early period of the disease.—In illustration of the importance of early medical treatment, an interesting fact is stated in one of the reports

from the government of Saratov, where the disease was in general virulent. On the estate of Count Gurjev, in that government, 166 persons were attacked, and 19 died without any treatment; but of the remaining 147, who were subjected to treatment at an early stage, 26 only were cut off, that is, little more than a sixth part.

We have said, that from the very commencement of the epidemic of 1830, it was almost universally regarded as a contagious disease. A very remarkable exception, indeed, occurred at Tiflis. Here, as already stated, the physicians were so satisfied of its non-contagious nature, that the inhabitants were encouraged to disperse themselves in the surrounding country. It is greatly to be regretted that the reasons for this bold measure, and still more its results, have not been touched on in the reports, for it might almost be regarded as an *experimentum crucis* upon the question of contagion. Every where else, however, the doctrine of contagion was unreservedly adopted, and means founded on this conviction were taken to check the progress of the disease.

Although the Reports supply few precise facts in support of its contagious nature, the brevity with which most of them are given will probably account for this deficiency, as it is impossible to conceive that the doctrine would have been so generally adopted without the daily occurrence of incidents considered favourable to this view of the question. The only facts of sufficient consequence to require special notice from us are contained in a full and interesting narrative of the appearance and dissemination of the epidemic in and around Astrachan by Dr. Solomov, staff-physician of that place. As his observations possess much interest, and relate to the first breaking out of the disease in the Russian dominions, we shall make no scruple in presenting them nearly in his own words—some passages only being omitted which appear to us immaterial.

"Cholera first appeared on the frontiers of the Astrachan Government on the 3d July, on board a ship of war, which had arrived from Baku, (350 miles down the Caspian,) and lay sixty miles from Astrachan. Till the 20th of the month the

disease was confined within the Sedlitovski Lazaretto, whither the vessel with the sick had been brought. But on that day four people were taken ill in the city, near the river Kutum; and from this point the disease imperceptibly spread over the whole town, carrying off a great number of people. After the 27th it attacked the suburbs, then the nearest villages, and then gradually extended over the whole Government. * * * In reaching Astrachan from Baku it passed over all the intermediate districts of the Russian territories. * * * The first places attacked after Astrachan were several Tartar villages in the immediate vicinity, at a distance of from two to four miles, the inhabitants of which were in constant communication with the town, and to which also many families fled out of it as the disease spread. On the 27th July it also appeared in the village of Tscherepacha, eight miles from Astrachan, on the return of some inhabitants, who had been to the town in search of work, and one of whom was the first person taken ill. After the 29th it proceeded through the Kossack stations and the town of Enotaevsk, on the highway to Moscow, up the stream of the Wolga, its extension in this direction evidently accompanying the fugitives from the places successively attacked. In the town of Enotaevsk it spread with the arrival of a sick boor. On the 29th July a barge arrived at Tchernojar, 150 miles up the Wolga, with several rowers on board, who were ill of cholera. On the 8th of August the disease began to prevail among the inhabitants, and then passed across the river among the neighbouring Kirghis, as well as upwards to the villages of Solodnikovsko and Vaisovka, in one of which the first person attacked was a military prisoner, who had been exposed to the disease. On the 25th July the epidemic also began in Krasnojar, situated on the northern mouth of the Wolga, twenty miles from Astrachan; and it first seized a private of invalids and a girl of thirteen years of age, who had both recently come from that capital. On the 3d of August it appeared in the estate of M. Nekrasov, ten miles from Krasnojar, and among the Algarin hills in the vicinity of the town; from which it finally extended down to the Cossack

cordon on the Caspian, between which and Krasnojar there is constant communication. While the disease prevailed in Astrachan some fishermen were there from Makovsky and Schitinsky, two places on the shore of the Caspian, where the Wolga opens into it. These men, terrified at the progress of the epidemic, hastened home, to place themselves, as they imagined, in security. But they had already imbibed the poison; some fell sick on the way, others after arriving at their homes; and the disease soon spread throughout the community to which they belonged.

"On the 2d of August the salt-depot of Basinsk in the Caucasan kingdom, ten miles off the highway, was subjected to the general pestilence. On the 1st of the month an Armenian, convalescent from cholera, arrived at the house of a private of the depot guard, who was taken ill and died next day; and other cases occurred afterward. At the salt-depot likewise of Kigatska, twenty miles from Krasnojar, a private soldier was taken ill whom I had sent thither from Astrachan, with medicines and instructions for the inhabitants, in case the disease should appear among them; and various individuals were attacked subsequently.

"Many gardens and farms in the neighbourhood of Astrachan remained exempt from the epidemic, having broken off all intercourse with the diseased districts. In many villages, too, where similar measures of security were taken, the issue was equally fortunate, although the cholera raged all around them, —for example, in the lordships of Smirnov, Beketov, and Prince Dolgoruki, in Sarepta, eight miles from Zaritzin, and some other places. On the other hand, the Kalmucks, who, as soon as the disease appeared among them, left their sick comrades behind them and repaired to pasturage-grounds fourteen miles off and more, did not in this way get rid of their fatal visitor: fresh cases occurred among the families who were first attacked."

After these interesting particulars he goes on to state, that in Astrachan many instances occurred where the greater part of the members of a family were taken ill in succession, and

that local causes appear to him inadequate to account for the disease prevailing epidemically, for the two seasons previous to 1830 were much more favourable than it to the extension of a disease of local origin, as the country was much flooded. He has unfortunately omitted to tell us in what manner the cholera was supposed to have passed into the city of Astrachan from the Sedlitovski Lazaretto; but he observes that suspicions were entertained of cases having occurred before the 20th of July, although they were not reported to the police.

Here we must leave the Russian Reports for the present. We wish that before doing so we could say they have enabled us to make up our minds on the great question for the elucidation of which they have been chiefly intended. But we cannot help declaring, that, in our opinion, while they certainly contain presumptive evidence of the Asiatic cholera being a contagious disease, they do not establish this point with the clearness which, in questions of the kind, the wariness and accuracy of medical science would desire in the present day.

The general tendency of the whole inquiry, as developed by the documents published by Professor Lichtenstadt, obviously leans toward the doctrine which some physicians in this country have within a few years been inclined to adopt in regard to all contagious diseases,—that the Asiatic cholera originates in a local cause, and for some time in this alone; but that it gradually acquires the power of communicating itself from man to man. Such is the doctrine espoused by Lichtenstadt himself. This is an easy mode of getting over the difficulties which have been encountered in attempting to solve the question of the contagiousness of various diseases; and, in the instance of cholera, it is peculiarly convenient, because there is no other disease, the phenomena of whose propagation are so perplexing to one who holds the doctrine of its origin in a single cause only. We conceive some of the facts mentioned in the preceding sketch are quite inexplicable, if it be assumed that the disease originates in terrestrial, atmospheric, or other local causes alone; that, on the contrary, other facts are equally incomprehensible, if the doctrine be upheld that it spreads only

by communication from one person to another; and, consequently, that it appears almost indispensable to allow the existence of both modes of propagation.

But, before adopting the view here taken, the question naturally arises, whether the facts now alluded to were carefully ascertained, and the collateral circumstances fully inquired into. In particular, it may be asked, whether implicit reliance can be placed in the numerous instances recorded, to the effect that cholera never appeared except where some one took the disease, after having recently been in a diseased district. We know that the supposed incidents of this nature, which have occurred to some medical officers in the East Indies, are fallacious; because instances have happened where, owing to the apathy or ignorance of the natives, the disease, though present in some districts, was not known to exist till it was discovered on the arrival of a European physician. And it is not saying too much to maintain that the southern and eastern states of the Russian empire are not yet so much elevated in their medico-political institutions, or in civilization generally, that similar sources of fallacy might not have been occasionally encountered there also, if diligently sought for.

We have had opportunities of mentioning, on various late occasions, our ideas on the accuracy of the tests by which it is usually supposed that a disease may be proved to be contagious or not; and have stated what, in our opinion, constitutes a better set of criterions than those commonly resorted to. We have not space to repeat here what has been previously stated. But we may mention that the kind of evidence, on which we conceive the greatest reliance may be placed, is nowhere to be found in the reports of the Russian cholera.

Taking, however, the Reports as they stand,—admitting the authenticity and precision of the facts,—it will not be easy to deny that a strong presumption is supplied of the disease having been contagious, whatever may be the fact as to its also originating in a different cause. Assuming, therefore, that it is contagious, which, it may well be imagined, the European states most liable to a visit from it, will do unreservedly, it is

requisite to add farther, that the Russian system of quarantine was not properly calculated to check and extirpate the disease. The duration of the Russian quarantine for the person is fourteen days. Now, the official documents supply the strongest presumption, that, if contagion is its only cause, the contagion may lurk in the body without breaking out for a much longer period. The latent period seems in general to have varied from two or three days to a week; but there are also facts which establish a probability that it may at times extend beyond thirty-five days. This is an important consideration, to be kept in view by those governments which may feel called on to adopt means for preventing the introduction of the pestilence, and to profit by Russian experience in their choice of measures.

Since the preceding sketch was finished, the British government has taken the alarm, and an Order in Council has been issued, subjecting certain articles from Russian and Prussian sea-ports to fourteen days' quarantine and purification. If the disease should approach any nearer the sea-ports on the Baltic, it will also be requisite to enforce quarantine for persons from suspected places; and indeed the Swedish government has already directed this measure to be resorted to, whenever vessels do not produce certificates of the town from which they sailed having been at the time free of cholera. When quarantine for the person shall be enforced, we may observe that it ought to be sharply looked after in this part of the kingdom particularly, as the voyage from the Baltic to the Firth of Forth is usually accomplished in a very few days.

The British government has, without doubt, acted wisely in considering epidemic cholera a contagious disease, so long as the only object is to prevent it from gaining access to our coasts. A little commercial inconvenience is a small price to pay for the chance of immunity. But it is to be hoped that the originators of the late order in council will not forget, that the question of contagion must not be solved in so summary a manner, if unhappily the disease should once gain a footing among us. The question would then be no longer a mere object of hypothetical speculation, which in practice might be solved in

the meantime, by holding what is called the safe side to be the true one. Before it is accurately determined whether or not cholera can propagate itself from one person to another, no man can say which opinion it is safest to hold. For in the one case the epidemic is to be checked by hemming in the population of the district where it appears; in the other by inducing them on the contrary to disperse over the country. The question, therefore, is not a mere subject of idle medical controversy, but is pregnant with the gravest practical conclusions.

Since we have been led to touch on the possibility of the cholera visiting the British Isles, it may be well to add, that our government should consider, that there is no other country in civilized Europe which has so great reason to dread the consequences of a visit from this terrible scourge. The density of our population,—the extraordinary quickness and extent of intercourse between our great towns,—the great difficulty which would be encountered in imposing restrictions on the liberty of the subject,—and the want of an adequate military force, if that difficulty were overcome,—would all combine, we apprehend, to render its diffusion swift and extensive to a degree unparallelled in its previous history.

REMARKS.

It does not appear to the writer that the origin of this dreadful disease is involved in such obscurity as the author of the preceding observations seems to think. The idea that contagion is the cause of the disease rests upon precisely such facts and such reasonings as, but a few years ago, were advanced in support of the doctrine of the contagious nature of the severe autumnal epidemics of our own country; and precisely such as a few years before that period caused most people, and very many physicians, to hold the doctrine that intermittent fever is contagious. Even Cleghorn asserts that the intermittent fever has as much claim to the character of a contagious disease as the small-pox. This error proceeded evidently from overlooking the truth, that the cause of a disease may proceed *from the circumstances in which the sick are situated*, as well as *from the*

persons of the sick: and as these causes equally elude the observation of the senses, and are only known to be present by their effects upon those who go where the sick are, physicians have fallen into the error of considering them in both cases as proceeding from the persons of the sick. The only way to settle the question with respect to a given disease is to separate the sick from those circumstances. If *wherever* the sick are carried, in *whatever* circumstances they may be, the disease appears among those who communicate with them, as in small-pox, we conclude that it is produced by a cause proceeding from the persons of the sick. But if in the absence of certain circumstances which are observed to be present or to have been lately present *wherever* the disease appears, *it never spreads when the sick are introduced from another place*, the disease cannot be produced by a cause proceeding from the persons of the sick, that is, cannot be contagious, but must be produced by some cause arising in those circumstances.

Let us apply these principles. Is small-pox produced by a cause proceeding from the persons of the sick? A person in this disease, in every variety of circumstances, will produce the disease in every person who is liable to take it, who communicates with him. It is therefore contagious.

Are autumnal diseases contagious? They arise in certain circumstances. Except these exist, they do not appear. Approaching the sick in multitudes of instances does not produce the disease; and communication with the sick removed out of those circumstances, never produces the disease. They are not therefore produced by a cause proceeding from the persons of the sick, that is, are not contagious, but are produced by a cause originating in the circumstances observed. These circumstances are the co-operation of heat and moisture on vegetable matter, and hence these diseases appear in low situations, near marshes, ponds, lakes, or rivers, in summer time.*

Autumnal epidemics have several forms, of which cholera is one. In every epidemic there are cases with more or less nausea, or vomiting, or purging, or all of these; and in some of

* See No. III of this Journal.

the cases these are so severe as to attract the principal attention and the name of cholera is given to the disease. In temperate climates the symptoms of cholera are not so violent as in tropical. But they differ only in degree: some of the cases in the tropical climates are like those in the temperate, being accompanied by a discharge of bilious matter; while in the temperate climates, at least in the southern part of the temperate zone, the cases frequently approach to the violence of those of the tropics, and the patient dies without having obtained the relief afforded by a bilious discharge. In both climates the internal state is the same, differing only in degree; in both a bilious discharge is salutary, and the continuance of it affords the best prospect of relief.

These cases are attributed to the same general cause with the other forms of disease prevalent in hot weather, because they appear together in the same circumstances; because the forming stage of the disease is the same; because the internal state is the same; and because bilious evacuations are salutary in all.

If the question be asked, why does the same remote cause produce this form of autumnal disease rather than any other, in some persons,—the answer is, because of some peculiarity of the state of the person affected. Variations in the operation of the same cause upon different people can proceed from nothing else. And when nearly all the sick are thus affected, it is because nearly all are then in that state which occasionally occurs and co-operates in the production of cholera. This disease, therefore, is the result of the same cause which produces other forms of autumnal or hot weather epidemics; and facts stated in the preceding paper abundantly show, that the cholera in Persia and Russia arose in the same circumstances in which such epidemics usually arise. This will fully appear on an examination of the district of country in which the places specified are situated.

The Caspian Sea extends from 37°, 15′ to 47°, 10′ N. Latitude. It is the lowest depression upon the surface of the earth, being 320 feet lower than the Black Sea. The Aral Sea lies

200 miles east of the Caspian, and the interval is so flat a country that the Jihon or Oxus which empties into the Aral, formerly discharged a part of its waters into the Caspian. This branch has been closed by the accumulations in its course, as has frequently occurred in branches of other rivers, as the Ganges and the Nile.

Through the great basin or depression surrounding these seas, run many large rivers, of which the principal is the Volga. This river is sixteen hundred miles in length, with a branch from the east, the Kama, five hundred, and one from the west, the Oka, six hundred miles long. The Volga rises in a country abounding in lakes, and two of its heads issue from lakes. The country drained by these rivers is so level that they are nearly exempt from falls or even from rapids. For four hundred miles, from Saratov to its mouth, the Volga receives no branches of any consequence, and discharges its waters by many outlets embracing a wide extent of country, which it enriches by annual inundations. This river passes through some of the finest provinces of the Russian empire.

The general course of the Ural, or Oural, a considerable river, corresponds with that of the principal eastern branch and the main body of the Volga, and empties its waters into the Caspian to the east of that river, by three mouths.

The Jihon drains the southern portion of the great basin above spoken of. It is about nine hundred miles in length and passes through one of the finest regions of the middle of Asia.

The situation of the great basin through which these rivers pass, is remarkable. The upper part of it, about the sources of the Volga, is marshy and full of lakes, affording an easy communication with the Baltic by several routes, by canals connecting this great river with lakes which discharge their water from one to the other and finally into the sea. The highest point of one of these routes is only 780 feet above the Caspian and 460 above the Baltic; so that, as before stated, the former is 320 feet below the level of the ocean. It follows, therefore, supposing the ascent from the former to the highest point in the course of the Volga to be regular, that a point near

the middle of its course is on a level with the ocean; and, the country for 400 miles from the mouth of that river being particularly level, that point may safely be fixed at the middle of its course. Thus a region of country around the Caspian, 800 miles from it in one direction, according to the course of the river, and of unascertained extent in other directions, but manifestly very considerable, the Aral sea, itself nearly on a level with the Caspian, being 200 miles from it on the east, is below the level of the ocean. Within this depressed region, on the Caspian Sea or upon some of the rivers emptying into it, is situated almost every place mentioned in the preceding paper, as attacked by Cholera. The peculiar circumstances of the situation of each and the time of the commencement of the Cholera shall now be stated. This disease raged with great violence in 1822 at Tabreez and the neighbouring country, and soon spread to Resched.

Tabreez is situated on a stream of water in a rich plain, not far from the lake of Urmia. This lake is to the west of the Caspian Sea, and on the shore of the latter is situated Resched, in a fertile plain. The disease broke out in July.

In 1829 the disease broke out at Orenburg on the 26th of August. This city is situated on the river Ural above mentioned as emptying into the Caspian Sea. The disease increased slowly for a fortnight, but after the 9th of September rapidly. From the 9th to the 25th of September there were 57 cases; from the 25th to the 30th, 75 cases; from October 1st to 21st, 615 cases; from October 21 to November 20, 353 cases; when it ceased entirely, having affected only 1100 out of 11000 people which the city contained.

These are precisely such circumstances as produce autumnal disease in every part of the earth in which the temperature is sufficient. This disease occurred at the very time autumnal disease is to be expected, and declined at the very time that it usually does.

In the same month, September, on the 23d the disease appeared at Rasupna, 60 miles west of Orenburg; on the 30th at Berdsk 12 miles North of the same city; "and between the 3d

and 8th of October, it also appeared in various villages and forts to the west and southwest, at a distance of between 12 and 33 miles."

Rasupna, the first place at which the disease appeared after it broke out at Orenburg, is on the same river, Ural, 60 miles below, (p. 403) Berdsk, the other place mentioned above, is 12 miles north of Orenburg; and at that city a branch of the Ural joins it from the North, so that Berdsk, which is a military station, is no doubt on that branch. The other places not named must also have been on or near the river, inasmuch as they lay to the west and southwest from Orenburg, and that is the course the river runs from that city.

At all these places the disease appeared about the same time, between the 23d *September* and the 8th of *October*.

The disease also appeared in places 200 miles north and northwest of Orenburg, which is the direction *upwards* of the river Ural and its branches. "The district of country visited, measures about 240 miles by 200; and was traversed between the 26th August and the 6th of February; but the greater part of it was visited before the middle of November. On the 23rd the disease was every where extinct." The circumstance of the disease not entirely ceasing with the coming of winter, has often been observed in autumnal epidemics. It will be mentioned again.

Before the disease appeared at Orenburg "it had been prevailing in several parts of eastern Persia, more especially in the province of Khorazan, and likewise in various districts of Bucharia, above all in Chiva, a city in the province of Kharazm, situated on the Jihon, a stream which falls from the south into the sea of Aral." (See p. 401) This too is just what might be expected of an autumnal disease. This country has already been mentioned as drained by the Jihon, and being more to the south by 9 or 10 degrees than Orenburg, the disease should be expected sooner. This circumstance will be particularly noticed presently as explaining the apparent progress of the disease northward.

The Cholera also appeared in some of the hordes of the

Kirghis "on the Ilek and Emba, the former of which streams joins the Oural from the southwest, a little below Orenburg, while the latter river runs parallel to the Oural at a distance of about 80 miles, and flows like it into the Caspian." (p. 402.)

In the year 1830 the cholera again made its appearance about the same season of the year in which it had begun in 1829. It appeared in *June* on the southern shore of the Caspian sea, especially in the towns, among which Resched was again attacked. (See p. 429.) It also appeared at various towns along the western shore, quite to the northern extremity of the sea, attacking Baku early in *July*, and Astrachan on the 19th of that month.

Baku is one of the best ports or havens on the Caspian. Astrachan is situated on an island in the mouth of the Volga, and of course surrounded by rich low grounds, which are annually inundated. (See p. 428.) The disease appeared at Astrachan several times before 1830. (See p. 394.) It appeared also at various points in the course of the river Kur, which empties its waters into the southern part of the Caspian. The only one named is Tifflis, the capital of Georgia, where it appeared on the 27th *July*. (p. 415.)

The cholera prevailed likewise at many points in the course of the Volga; at Erotaevsk, Tschernojar, Zaritzin, and Saratov. These are towns on the Volga, all on that part of its course above mentioned as being through a rich country, so level that it receives scarce a single branch of any consequence. (p. 428.) At Zaritzin the disease appeared on the 4th, and at Saratov on the 6th of *August*.

The next places mentioned are the country of the Don Cossacks, Kharcov in the government of Woronesch, and the government of Kiev. (p. 416.) Through this region the Dnieper, and the Don with its numerous branches pass into the Black sea. The Dnieper heads amidst marshes and lakes, and passes through a country so level as to have scarce a hill of any considerable consequence. The navigation of this river is interrupted in one place only, in which there is a series of rapids, but so slight that in the spring loaded boats pass them.

On this river is situated the town of Kiev. Between this flat country and that about the mouth of the Volga, the Don takes its course. On a branch of this river Kharcov is seated. The disease appeared in this country in *September*.

"But the main stream of the epidemic continuing *to follow upwards the channel of the Volga* from Saratov, turned north-eastward with the river, and arrived (*appeared*) at Samara, on the 27th of *August*." (See p. 416.) Samara is situated at the conflux of two rivers not far from their entrance into the Volga.

Near the same time, on the 17th of *August* it appeared at Penza on a considerable branch of the Volga. At Nischnei-Novgorod, situated at the junction of the same branch with the Volga, the disease appeared about the end of *August*. On the 9th *September* it broke out at Kasan on the Volga, 200 miles below Nischnei-Novgorod. About the same time also it appeared at Kostroma, also on that river; and in the governments of Vologda, Novgorod, and Tver, a country of lakes and rivers, among which the Volga and its branches are conspicuous. It also attacked places on a tributary of the Volga, coming from the southwest and falling in at Nischnei-Novgorod, the river Oka; and appeared at Moscow on a branch of that river towards the middle of *September*, and between that time and the 10th November five or six thousand people were attacked. The situation of these towns is not minutely stated in general in the geographical works, but their condition may be judged of by that of Moscow. Its suburbs contain gardens, cornfields, much open pasture, and some small lakes which give rise to the Neglina. The Moskwa flows through the city in a winding channel, but excepting in spring is only navigable for rafts. It receives the Yausa in one division of the city, and the Neglina in another, and both of these are nearly dry in summer. It has also a canal communicating with the Don.

The account of Dr. Somolov of "the appearance and dissemination of the epidemic in and around Astrachan," bears a striking resemblance to many accounts to be seen in our medical journals of the first years of the present century and the

last of the preceding, relative to the appearance and prevalence of the severe autumnal epidemics of those times. (See p. 420.)

The present seat of war in Poland abounds in marshes and rivers. Warsaw and Praga are on the Vistula, which communicates by a canal with the Dnieper, mentioned above as passing through a flat country. Petersburg, too, was built in the midst of marshes. The northern part of Europe has often been the seat of epidemic fevers.*

It appears, then, that all the places mentioned, in the preceding paper, are so situated as to give reason to expect autumnal diseases in the proper season; and that this form of autumnal disease, cholera, broke out at the very season of the year when they are to be expected—viz. in July, August and September. (See pp. 429 to 432.)

It is stated, however, that the disease in some places continued after the frosts set in, and even commenced in some instances after the weather was very cold. This it has been thought shows that the cholera does not depend upon miasmata, inasmuch as "it is well known that terrestrial miasmata, so far as we have hitherto been acquainted with their properties, cannot exist under such conditions." (See p. 412.) But the very same thing has occurred in diseases which are certainly dependant for their existence upon the concurrent action of heat and moisture on vegetable matter. The common autumnal fevers frequently continue after cold weather sets in. Cleghorn says he has observed them to prevail in Minorca until the month of January; and mentions the occurrence at the same time of many other cases with bilious vomitings and purgings together with pleuritic symptoms.† Sydenham mentions similar occurrences in England; and says of the winter fever of 1675, which had in addition to the symptoms of the autumnal fever, a cough on the coming of cold weather, "The fever was the very same with that which prevailed to the day when

* See Pringle's Dis. of the army pp. 178, 180. He mentions terrible epidemics in Hungary and in Copenhagen.

† Cleghorn's Dis. of Minorca, pp. 238, 242.

this cough first appeared."* Rush mentions the same. He says of the autumnal fever of 1794 in Philadelphia, "The cold weather in October checked the fever, but did not banish it from the city. It appeared in November, and in all the succeeding winter and spring months."† So also several cases of malignant bilious fever appeared in March and April following the severe autumnal fever of 1795.‡ Similar occurrences are mentioned in other winters, frequently with the addition of pains in the thorax, constituting bilious pleurisy, and bilious catarrhal fever. After the severe fever of 1797, there were catarrhs and bilious pleurisies, in November, December, February and March.§ *Bilious* pleurisies were also common in February following the severe autumnal epidemic of 1798. though the winter was extremely cold.‖ In January following the severe epidemic of 1799 also, bilious fevers with and without pleuritic symptoms occurred.¶ In January and February following the epidemic of 1802 there were catarrhs and bilious pleurisies, with a tertian type.** In February, March and April following the epidemic of 1803 bilious pleurisies were common.†† Bilious catarrhs and pleurisies were common in January, February and March following the autumnal season of 1804 in which intermittents were very common in the western part of the city.‡‡ The dependance of these winter fevers on the same cause which produces the autumnal diseases, is shown not only by the bilious vomitings and purgings which occurred in them, but by the health of those winters which followed healthy autumns. Thus, the autumn of 1796 was uncommonly healthy and the winter was also.§§ In 1800, although there were a few cases of malignant fever in one place produced by a limited or temporary cause, the city in general was remarkably healthy, so that Dr. Rush endeavours to account for it; and the winter also was uncommonly free from disease.‖‖ In the autumn of 1801 also the city was healthy;

* Sydenham's Works, sect. 5, chap. 5.
† Rush's Works vol. iii, p. 199.
‡ Ibid. p. 243.
§ Ibid. vol. iv, p. 39.
‖ Rush's Works vol. iv, p. 55.
¶ Ibid. p. 63.
** Ibid. p. 83.
†† Ibid. p. 91.
‡‡ Ibid. p. 95.
§§ Ibid. vol. iii, p. 244; vol. iv, p. 3.
‖‖ Ibid. vol. iv, p. 64, 65, &c.

and in the winter no disease except the measles. Every man of extensive experience may have observed similar occurrences.

The following statements from the New York Med. Repository set this matter in a very clear and strong light. They are taken from letters of Dr. Barker to the editors, respecting the epidemics of several years in and about Portland in Maine, in the neighbourhood of which there is marshy land and one or more streams of water.

About the middle of August 1797 an epidemic fever and dysentery commenced and became very general; but did not entirely cease on the coming of winter, and the fever continued until March.*

In the following year, 1798, the summer was unusually hot and sultry. From the beginning of July to the last of February 1799, an epidemic fever was very prevalent in the neighbourhood. Dr. Barker attended 6 cases in July, 16 in August, 25 in September, 24 in October, 9 in November, 8 in December, 15 in January, and 6 in February.†

These cases show beyond all question that miasmatic diseases do sometimes continue to prevail in a season in which miasmata themselves cannot exist. The reason of this, it appears to the writer, is plain. The impression made on the system by the cause of autumnal diseases is not instantaneous, but gradual. The system by its action is brought into a disordered state. That state being present, any occasional cause of fever will produce it, and the fever produced is proportioned in violence not to the action of the cause, but to the state of the patient. In such a season an exposure which at other times would produce no bad effect, will cause a severe attack of the prevailing epidemic. By avoiding all the causes of fever, called occasional causes, a man may escape entirely, notwithstanding the existence in him of the morbid state produced by the prevalent cause. But inasmuch as he may at any time of the autumnal season, to its very close, suffer an attack of fever on exposure to the occasional causes, it is evident that the morbid

* Med. Repository, N. York, vol. ii. p. 140. † Ibid. vol. iii, pp. 364, 365.

state exists until the close of the season; that is to say, until the coming of cold weather. And then the question is, how much longer will he retain it? This question is to be determined by observing *how long he continues liable to such an attack* upon exposure to the occasional causes; and observation has shown that many continue liable throughout the winter. The reason therefore that miasmatic diseases are observed to prevail in seasons in which miasmata cannot exist, is that the morbid state produced by miasmata, which precedes an actual attack, continues for a considerable time, and therefore may exist in persons who have been exposed to their influence after the cause itself, miasmata, has ceased to exist. And as individuals frequently escape an attack until after the cause, miasmata, has ceased to exist, it is evidently possible that every one shall escape until that time, in which case the epidemic would commence in cold weather. The following is an instance of the actual occurrence of this.

Bethlehem township, Connecticut, four or five miles square, is ridged into hills running due north and south. There are six of these ridges, and in each valley there is a stream of water. The soil is very fertile. It was settled in 1730, and continued healthy until 1750. In 1748 a man built a dam across the central valley, in order to kill the timber on a piece of ground containing about 50 acres. The ground continued covered until the spring of 1750. The water was then let off, and was followed by a pestilence which carried off a number of the most robust inhabitants. The summer was a very hot one.

As few of the physicians, and still fewer of the people, thought the ponded swamp the cause of the disease, ten years after what the old people called the first great sickness, the same low ground was again overflowed. After the letting out of the water in 1760, another wasting sickness began in the mouth of November, and carried off about forty inhabitants.*

It is evident from these statements, that miasmatic diseases do sometimes continue in cold weather, and therefore the continu-

* N. Y. Med. Repository, vol. i. p. 513. See No. iv. p. 557 of this Journal.

ance of Cholera in winter in some places is no valid objection to the doctrine that it is such a disease. On the other hand, the prevalence of this epidemic in places in which miasmata must abound in the proper season, its commencement at the very season in which they do abound in all places suitable to their production, and its decline at that season in which miasmatic diseases usually decline, together with the occurrence of the disease in connexion with such diseases on many occasions, show that it is itself such a disease.

Another objection to attributing this disease to miasmata is founded on the fine situation of Orenburg on the higher bank of a fine river, on a plain free from bogs, marshes, traversed by clear streams, &c. "How any baneful terrestrial emanations could be engendered in such a district it is impossible to imagine;" says the writer, (p. 411.) But we every year, in just such places, meet with the same autumnal diseases which occur in places more abounding in vegetation undergoing the process of decomposition, only less violent and less numerous in correspondence with the smaller amount of vegetation. We must conclude, therefore, that they are produced by the same cause; and we have evidence that there were abundant materials for the production of miasmata at Orenburg in the autumn of the year in which the Cholera prevailed at that place: for we are told that "there was an unusual abundance of fruits, more especially of gourds and water-melons," which state of things indicates abounding vegetation in that summer. (p. 412.)

Another particular thought worth mentioning as an objection to the doctrine that Cholera is a miasmatic disease is, that it appeared in villages considerably elevated above the general surface of the plain in which it prevailed. In the East Indies, it is observed, the Cholera does not appear in the mountains. This and other facts are mentioned as rendering it difficult to trace this disease to terrestrial emanations. The reason, however, why Cholera does not appear in the mountains of India is, that they are not in such circumstances as to produce miasmata. But this is no reason why high ground that is in such circumstances should not produce them, and the diseases flow-

ing from them. The mountainous coasts of Malabar and Coromandel are dry and sandy and the rivers are small, and the whole coast is healthy in comparison with other parts of India.* These situations have always been noted as most free from miasmatic diseases of every form.† But that these diseases do appear in elevated situations when there is moisture and heat is certain.‡

These objections therefore are entirely insufficient to invalidate the conclusion that Cholera is produced by miasmata, and on the other hand there are many strong facts in support of this doctrine.

The epidemics of India have been mentioned by many writers and attributed to the true cause, the heat and moisture of the climate operating upon its abounding vegetation; and Cholera has been mentioned as one of the forms of these epidemics. In that of 1762 which is described as a remittent fever by Lind, there were cases of Cholera. "In this sickness (he says) a constant vomiting of a tough white pellucid phlegm, accompanied with a continual diarrhœa, was deemed the most mortal symptom."§ The same writer, in speaking elsewhere of the causes which bring on an attack of the various forms, mentions their having sometimes occasioned a violent vomiting and purging which have "carried off the patient in twenty-four hours."‖ In his chapter on the cure of these diseases he speaks of cholera as one of the forms, and mentions it by that name which indicates the severest grade, *mort de chien*.¶ Pringle, also, speaking of the diseases of Java in the East Indies and attributing them to the action of the heat on the marshes, says of them, "The distempers are the *Cholera*, flux, and a continued putrid fever."** It is manifest, therefore, that cholera has always prevailed to a greater or less extent in company with the other forms of hot weather, commonly called autumnal, diseases, in India; and that those places which are remarkable for its prevalence now are the very same in which it was always

* Lind on Hot Climates, p. 65. † See Lind, pp. 152, 153, 154, 156, 157, 161.
‡ See No. iii, p. 349, &c. of this Journal. § Ibid. p. 62. ‖ Ibid. p. 130.
¶ Ibid. p. 191. ** Diseases of the Army, p. 195.

observed and in which miasmata abound. So in like manner, the places in which this disease has prevailed in Russia and Persia, abound in miasmata at the proper season, and have heretofore been the seat of miasmatic fevers; among which, too, cholera made its appearance before 1829, as in Tabreez and Resched in 1822, and in Astrachan in several different years. (pp. 429, 431.)

The fact related of the universal indisposition of the inhabitants of Orenburg while the cholera prevailed, is strongly in favour of the doctrine of the miasmatic origin of the disease. Scarce a single person escaped. "One complained of oppression and pain in the breast; another of headach, slight sickness, looseness of the bowels, and the like." (p. 413.) This shows the operation of a cause upon the whole population, which therefore must have been in the atmosphere, and the symptoms mentioned are precisely those which are observed during the prevalence of autumnal epidemics.

Another circumstance strongly supporting the doctrine of the miasmatic origin of the disease, is its appearance in the same season in different years, and that the season of miasmata. (See pp. 394, 395, 397, 415, 416.)

Another is its appearing over such an extent of country almost at the same time, showing the operation of a cause acting upon the whole at once; viz. the heat of the sun upon the moisture and vegetation of the whole country. (See pp. 415, 416.) The difference in the time of the appearance at different points corresponds very well with the difference of temperature arising out of the difference of latitude. Thus, the disease appeared at Resched and the neighbouring country on the southern shore of the Caspian sea in Lat. 37½° in *June;* at Baku another port on the Caspian 200 miles north of Resched early in July; at Astrachan at the northern extremity of the Caspian, about 400 miles north of Baku on the 19th July; at Zaritzin about 250 miles north of Astrachan on the 4th of August; at Saratov 200 miles further up the Volga and further north, on the 6th August, and at Samara 200 miles northeast of Saratov on the 27th of August. (See pp. 415, 416.)

Another is the rapid manner in which the disease spread. Thus in Tifflis 579 were seized in ten days; in Baku 1229 in the same number of days; at Saratov 2367 died in one month; and in Penza 1200 in 14 days. No cause which had not access to the whole population at the same time could so rapidly affect so many. (See pp. 415, 416.)

Another is the suddenness with which this immense amount of sickness ceases, rapidly declining with the decline of the production of miasmata, and for the most part ceasing when it ceases. (See pp. 396, 397, 407.)

Let us now inquire what facts are alleged in support of the doctrine of the contagious nature of Cholera.

It was supposed that the Cholera was introduced into Orenburg by the caravans which arrived there from the country southeast of the Caspian sea where the disease was prevailing, at Chiva, &c. (p. 430.) It appears, however, that the last caravan arrived on the 22nd July, five weeks before the first case occurred in Orenburg; and even those who believe the disease to be contagious, admit that communication with those who have been exposed to it is entirely safe in two weeks, and have therefore fixed the time of quarantine at fourteen days:* moreover, the caravan was in perfect health; they had lost but one man on their long route, of an illness of twenty days continuance, and therefore not the Cholera; the goods had been opened at every resting place on the journey, and no inconvenience was experienced by the carriers; and no one who purchased from them was attacked by Cholera; and lastly at the towns on the route, the Cholera did not appear during the season. It is therefore impossible that the disease could have been produced in this way. (See the remarks of Dr. Somolov, p. 402.)

It is stated as an important fact in favour of contagion "that the Cholera broke out in some places very soon after the arrival of persons ill of Cholera who had recently left districts

* "In all the cases when the disease broke out soon after the sickening of a person newly arrived from a diseased district, it appeared among the residents in less than a week." (See p. 408.)

where it prevailed. "Thus, at Rasupna the first person attacked was a visitner's servant on the day he arrived from Orenburg, the 19th of September, and the disease in four days afterwards attacked several of the garrison." (p. 404.) But it is not even stated that this man had been with the sick at Orenburg: when he left that city, about the 17th, there were not probably, in a population of 11000, more than 20 cases: (See p. 395.) and lastly, not one of those who were attacked had been exposed to the contagion supposed to have been introduced by him, and of those who visited him not one was attacked. (p. 407.) In another case a soldier and a soldier's wife went from Orenburg to Iletsk, were taken ill on the road, and died in a day or two after their arrival at the latter place. Three days after their death three persons were taken ill in the garrison. The husband of the woman and two girls who visited the soldier soon after his arrival from Orenburg. The aunt of the girls who nursed them, was next attacked, and after her her two sons. (pp. 404, 407.) But these two girls were not the only persons who were exposed to the contagion, if there was any. Who nursed the soldiers? Why is it not stated that they were taken ill, if they were so. The plain presumption is that they were not, and as they were not the girls did not derive their illness from him, but from the same cause which attacked the whole family, the girls and their aunt and her two sons. One other case is mentioned, (p. 404) of a village remaining healthy until a man arrived from another village which was sickly, after which the former became sickly, the first cases occurring in the family of the visiter. This is precisely such evidence as was often brought a few years ago to prove that our autumnal fevers are contagious—such as the writer has heard earnestly insisted on as proving it, and which does prove it if such evidence can prove a disease to be contagious. In the numberless instances of villages attacked in the immense extent of country over which the disease prevailed, it would be wonderful if some such coincidences did not happen, as the breaking out of the disease soon after the arrival of a person from a place where it was prevailing. But if this was really

caused by his carrying the disease and spreading it by contagion, there would be no want of evidence on the subject when so many villages and towns were attacked, inasmuch as if communicated by contagion it could be clearly shown in many cases. There would be no difficulty in tracing the small-pox from one to another if that disease had been introduced. There would be found none with the disease who had not been exposed, and none exposed without the disease.

The irregularity of its course, or rather of the times of its appearance at different points, has been considered as favouring the doctrine of contagion. The irregularity spoken of (p. 405) agrees, however, very well with the manner of the appearance of autumnal diseases, which break out here or there according as the circumstances producing them are more or less favourable at different points. Thus at Baku on the shore of the Caspian in Lat. 40° the Cholera appeared early in July; at Astrachan at the mouth of the Volga in Lat. 46° on the 19th July; while at Tifflis on the Kur, situated high up in its course, it did not appear until the 27th of the same month, though in Lat. 42°.

Another fact mentioned as favouring the doctrine of contagion is the escape of some villages the inhabitants of which cut off all communication with the surrounding country. This is at best but negative testimony, while the escape of *many* which did not shut themselves up in this manner is positive, and proves that the former did not owe their safety to shutting up.

It is obvious, therefore, that these reasons are far from being sufficient to convince us of the contagious nature of Cholera; and there are, moreover, a number of facts totally subversive of that doctrine, related in the preceding account of the disease.

First, the rapidity with which it occupied the whole country. An extensive district about Orenburg was overspread in fourteen days, from the 23d September to the 8th October. (p. 397.) The country around the Caspian and up the Volga was occupied by it in a space of time utterly too short for the *progress* of any contagion we know any thing about. The progress up

the Volga is attributed by those who believe the disease to be contagious, to the arrival of sick boatmen at the different towns on that river. [p. 419, &c.] But the disease appeared at Zaritzin on the 4th of August and *two days afterwards* at Saratov *two hundred miles higher up the river*. This is too short a time for it to have been carried up. It must have arisen in each place out of the circumstances of the place. The same cause which made it appear at Resched in June, at Baku &c. in July, gave rise to it at Zaritzin on the 4th and at Saratov on the 6th of August. These accounts of the manner in which the Cholera spread, are strikingly like those of the spreading of the fever in Philadelphia in 1793 by Dr. Rush; and just such evidence might easily be produced of the contagious nature of the epidemics of every place and of every year from that time to this.

Again, if contagion were the cause of the spreading of Cholera, why did it not fill the country in 1822, when it was at Tabreez and Resched? Why did it not spread up the Volga, from Astrachan in the several years in which it appeared in that city before 1830?

The fact stated in page 407 respecting the prevalence of the disease at Rasupna is alone enough to prove the disease is not contagious. That stated in the same page of the appearance of the disease at Tschernoretschinsk, five weeks after it had entirely disappeared in all the neighbouring villages, is of the same character. That stated in page 409 respecting the prevalence of the disease at Orenburg, is also of itself enough to settle the question of contagion. So also is that stated of the disease in Tifflis. (p. 415.) In ten days 579 persons were attacked and 237 died: but notwithstanding the most unreserved communication among the people and their scattering over all the neighbouring hills, we have no account of the increased spread of the disease in the town or among the inhabitants of the neighbouring villages. Contrast this absence of the disease *among the hills around Tifflis*, notwithstanding the most unreserved communication, with the spread of the disease *in the villages in the low grounds of the Volga*, notwithstanding the belief

prevalent there of its contagious nature, and the consequent caution of the people against exposure to the supposed contagion. (pp. 415 & 420.)

Moreover, the rapid manner in which the people became sick is totally inconsistent with the spreading of the disease by contagion. A sick person arrives in a place and in a few days *five, ten or fifteen hundred* are ill. (See pp. 415, 416) Indeed it is expressly stated that scarcely a single inhabitant of the city of Orenburg entirely escaped the influence of the cause of the epidemic. (p. 413.) This universal disorder shows the operation of a cause upon the whole population, which *therefore* was not contagion.

Another objection equally strong, is that notwithstanding this rapid progress of the disease, so small a part of the population are affected. In Orenburg only 1100 out of 11000. A contagious disease capable of so wide spread an influence as to affect a thousand persons in ten days, would assuredly not stop short in its course after affecting only *one tenth part* of the population of a city.

Lastly, it is an insuperable objection to the doctrine of contagion that the disease after being spread over an immense region ceases in cold weather. Thus, in Orenburg, in 1829 there were 612 cases in the first three weeks of October, and in the following four weeks 353 cases more; but yet at the close of those four weeks, on the 20th of November, the disease had entirely disappeared from the city and suburbs. (p. 397) Moreover, a region around the city 240 miles by 200 was overspread by the disease in September and October, and yet it had almost completely disappeared before the end of November. (pp. 405, 407)

It has been shown that the disease in question arises in situations and at times in which miasmatic diseases arise; that the arguments for supposing the disease to be spread by contagion are not sufficient, and that this doctrine is totally inconsistent with the facts observed in the rise, prevalence and cessation of the disease.

Beset on one hand by the insuperable objections to the doc-

trine of contagion observed in some places, and on the other by supposed difficulties in the way of the doctrine of the miasmatic origin of the disease, (pp. 433, 437) some have had recourse to a double origin—supposing that the Cholera may originate in a local cause, but that it acquires afterwards the power of communicating itself. (p. 422.)

Supposing this a possible thing, the propagation of the disease by an acquired contagious quality would be liable to most of the objections above stated as fully as if it was originally a contagious disease. But this, if not impossible, is at least such a thing as we can never ascertain. In order to determine the question of the origin of a given disease, say small-pox, let us suppose the term contagion not yet invented. Those who are engaged in the investigation observe that those who go among the sick take the disease. This affords ground for suspecting that the cause of the disease proceeds from the persons of the sick. It would still, however, be suspicion only, and *it might proceed* from the circumstances in which the sick are situated. The question, which of the two is the source, is to be decided only by separating the sick from the circumstances in which they are placed. Suppose this to be done, and it turns out, that *wherever the sick go* the well are affected; no circumstances of heat or cold, of dryness or moisture, of habits or condition in life, making any material difference; whoever goes near the sick takes the disease, and nobody ever without going near them. We are, then, driven to the conclusion that something emanating from the sick produces the disease; something of which we know nothing, which eludes our closest observation, and *the very presence of which we can never ascertain but by its effects.* To this conclusion we are not led, it will be observed, *by any positive evidence of the existence of such a cause*; but *we are driven to it* by the impossibility of discovering any other cause *to which we can attribute the disease.* The proof lies not in showing that the cause of disease actually proceeds from the sick, but in showing that every other conceivable source is cut off, and this only is left to us: this we must admit, because there is no other. From this it follows immediately that as

long as there is a cause to which the disease can with any appearance of reason be attributed, as long as the appearance of the disease is connected with any set of circumstances, we are not under the necessity of having recourse to the last resort, contagion. It follows also that we cannot with any propriety conclude that a disease is propagated sometimes by contagion and sometimes by another cause. For, if ever another cause is shown to exist in the circumstances in which the diseased are placed, it strikes at the root of the doctrine of contagion, which rests only *upon the utter exclusion of every other cause.*

As to the cure of Cholera, a few words only shall be offered to the reader. Every symptom shows that the circulating fluid has left the outer parts of the body, and is of course accumulated in the interior. This state of things is to be removed by exciting the action of the heart so as to carry forward the blood accumulated in the great interior veins, and by producing evacuations from the liver, which immediately lessen the interior accumulation because a large portion of the accumulated blood is in the vessels of the liver.

The former is attempted by brandy, tincture of opium, &c. But vomiting is a much more effectual means of rousing the circulation in most cases in which the action of the heart is languid and the blood has left the surface. This, too, is the very means employed by nature, the very mode in which the constitution makes an effort to remove the morbid state in question, in perhaps most cases of autumnal disease. When this effort is successful the case is not called Cholera; but only when the morbid state is past relief from this remedy. When the patient continues to present every appearance of languid circulation and of interior accumulation notwithstanding the operation of the most effectual of all excitants, vomiting, the inferior remedies commonly employed to stimulate, have but a poor chance of succeeding, and often fail when most freely used. The chief hope lies in the production of evacuations from the liver; and in this way also does the constitution often make an effort to relieve the system from the accumulation with-

in. Experience shows that spontaneous evacuations of bilious matter are favorable and salutary; and that when artificially produced and continued, they seldom fail to relieve the patient.

The remedy best adapted to effect this purpose is calomel; and it is generally retained, if given properly, because it is tasteless, small in bulk, and weighty. The best way to administer it is mixed with a fluid not too thick. If given in form of a pill or bolus, the stomach is often enabled to reject it; also if mixed with jelly or preserved fruit, with which it forms a mass easily thrown up. With milk or milk and water or thin syrup it is not often rejected. As the time for efforts to save the patient is short, the physician should aim to give at once a dose sufficient to operate. Many will venture upon ten grain doses every hour or two in cases of emergency, who will reject the idea of a drachm at once. But on every account *in such cases* the latter dose is preferable. It is more effectual in a given time, say twelve hours, and without speedy relief the patient's *hours* are numbered. It takes less to operate in this way, because a full dose if retained operates sooner than divided doses. It is, also, more certain to operate, and for both these reasons less apt to salivate. Salivation, however, is not to be apprehended in these cases; and therefore the chief objection to the use of calomel is removed. No doubt in many cases a smaller dose would be effectual; but it would often fail, and it is better to give a little more than necessary in some cases than too little in others, in a disease so rapid in its progress and so fatal as Cholera. The two modes have been tried on the same patient in different attacks, five years apart, in the latter of which he was not expected to survive many hours. In the former twenty grain doses were relied on with the aid of stimulants:—the patient was relieved and recovered, but the contest continued for several days. In the latter a drachm of calomel was given and nothing else that day. In twelve hours there was a copious flow of black bile and the patient was completely relieved from the exhausting discharges of serous fluid and other symptoms. The next morning he was obviously out of danger, and the discharges of bilious matter being kept up

by aloes, calomel and rhubarb in pills, the patient rapidly and perfectly recovered. This patient was a man aged about thirty-five.

At the same time a boy of about fourteen was attacked, and the writer was called in consultation after he had taken two twenty grain doses of calomel. It was not thought advisable to propose an increase of the dose. But very copious thin and colourless discharges continuing, he took the same dose three times a day. In the night of the second day he had several copious discharges of bilious matter and was greatly relieved. But this was made the ground of reducing the dose of calomel to one half and adding aloes, making aloes and calomel ten and ten, contrary to the opinion of the writer; and the consequence was that in a few hours the bilious discharges ceased and the serous returned, and the insufficient practice being continued he died the next day. At the same time a girl of ten being attacked, she was advised to take 10 grains calomel every hour until they brought away bilious discharges. She took six doses; the discharges became black and being kept up by cathartic medicines without calomel she rapidly recovered. The calomel having effected a free and thorough secretion and discharge of bile, it was laid aside as unnecessary and as liable by its continuance to affect the mouth. But had the discharges shown a disposition to become serous and colourless, as in the former case, it would have been necessary to have returned to the calomel or let the patient die.

It is obvious that there is nothing in this practice to prevent the use of stimulants at the same time; and unquestionably they have often been used with excellent effect. It may even be that in the severer cases the calomel alone would not be effectual, on account of the rapidity of the disease. But the writer is persuaded that it is the most important remedy, and that stimulants are secondary.

ART. VI.—*Remarks on the Appendix to Professor Caldwell's paper on the Original Locality of Fever, in the last number of this Journal.* By JOHN E. COOKE, M. D.

THE Appendix to the paper of Professor Caldwell in the last number of this Journal was not seen by the writer of the following remarks until that number was published; he having been absent at the time that part of the work was put into the hands of the printer. The first opportunity is embraced to make a few observations respecting it. The first particular to which the reader's attention is called, is the colour of the blood in persons exposed to the action of miasmata. The writer's observation has led him to believe that it is darker than common. Professor Caldwell asserts the contrary, and appeals to high authority in support of his assertion. There seems, however, to be a misapprehension of the meaning of the writer on this subject. The quotations from Rush, Cleghorn, &c. show that the blood in the severe autumnal fevers described, was of a scarlet colour. This the writer does not question. He has often noticed it himself. It is not *in fever* that the darkening of the blood is spoken of as produced by the interference of miasmata. It is previously to the production of fever, and leads to it; although the blood sometimes continues dark in fever, as in fevers without much action of the heart and arteries. This meaning of the writer might have been perceived in the quotation from his paper in the Appendix, (p. 221) viz. "*every body* has the blood more or less darker than usual when miasmata are mixed with the air we breathe." This is observed on bleeding those who have not been attacked with fever, who for various aches and pains choose at such time to be bled. This is stated as a matter of observation. No doubt it is possible there may have been error in making it, but this can only be decided by an appeal to further observation. As to the argument (p. 222) against the *possibility* of the interference of miasmata with the due action of atmospheric air, no other re-

ply is necessary than that a priori arguments against the truth of an alleged fact, are not to be trusted where appeal to observation is so easy.

Professor Caldwell is induced by a passage in the paper of the writer on the doctrine of Broussais, in the 13th number of this Journal, to believe that the writer "does not correctly understand the doctrine he opposes. Hence (he says) his error. It is not contended, certainly *I do not contend*, that the local morbid affection which results in bilious fever, is inflammatory from its commencement." (p. 223) In reply to this suffice it to remark, that the paper was written against the doctrine of Broussais, not of Professor Caldwell. *If the former does not* hold the doctrine attributed to him, the writer with a multitude of others, has *misapprehended* him.

To the remarks on the subject of fever, the writer is not disposed to reply. He is content for the present at least to leave the reader to the consideration of the paper alluded to and the remarks upon it. (See No. 13, p. 103 & No. 14, p. 221.)

ART. VII.—*Minutes of the proceedings of the Medical Society of Tennessee, at the second annual meeting, held in the City of Nashville, May* 1831. pp. 29. Nashville, 1831.

IN addition to the "Minutes" of the Society, this pamphlet contains an interesting "Oration," of which we shall speak more particularly hereafter. The subject of it is "Medical emulation;" and it was "pronounced before the Medical Society of the State of Tennessee, May 3d. 1831, by John H. Kain, A. M., member of the Society."

Of the establishment of State Medical Societies, on suitable principles, and for the attainment of the ends which naturally belong to them, we have always thought highly. They constitute one of the most effectual means that can be devised for promoting the interests of the profession, and, through

them, the welfare of the community. They may be made an instrument, if any thing can, to crush quackery, with its frauds and dishonors, and all its forms of moral turpitude, and to protect the people from great suffering by its arts and impostures, and the country from incalculable losses of human life. They are also perhaps the only sources, from which a code of sound medical ethics can be made to overspread the nation, and by which a due observance of it can be secured. They can be also alone competent to furnish full and satisfactory views of all our endemic and epidemic diseases, as connected with topography and meteorology, and to provid ethe materials necessary for a complete system of medical statistics, for the United States. Nor must we forget another effect peculiarly beneficial to American medicine, which they may be made to produce. We allude to the encouragement they might give to medical writings in all parts of our country. Under suitable regulations, they might be made to insure the sale of an edition or two of any valuable professional work, from an American pen. On the advantages of such an arrangement it would be superfluous to dilate. It would, in a short time, not only call forth more creditable exhibitions of American mind, on subjects of the Profession, than have heretofore appeared, but, by the general diffusion of native productions, it would throw all the scientific and practical knowledge of our country into a common stock. On this every physician might draw at pleasure, much to his own improvement and credit, and to the benefit of the sick. Even Schools of medicine might be made to feel extensively and advantageously the influence of a general establishment of State Medical Societies. The education of young men, designed for the profession, might be rendered much more thorough and substantial by it, and therefore more worthy of the responsible station they mean to occupy.

Of many of these advantages, if not of the whole of them, the Medical Society of Tennessee is fully sensible, and is pursuing measures calculated to attain them. The institution has commenced its career, with a degree of ardour and resolution, and an amount of intelligence, which cannot fail to be pro-

ductive of much good. Its meetings are to be annual, held in the month of May, of every year, and at each of them an oration is to be delivered, by a member previously appointed for the purpose, and three papers, on medical topography and the diseases of the places to which they relate, read by members resident in the three great divisions of the State. Of these regulations it would be unjust to say less, than that they are marked by wisdom and practical judgment. The orations will sustain the spirit of the Society, and point out new and useful objects of pursuit, and the papers, besides exercising their authors in composition, and rendering them more vigilant and accurate in observation, and communicating much interesting information to their fellow members, will furnish matter to enter hereafter into a work on the medical statistics of the State. Under such influences it may be pronounced impossible for the Profession in Tennessee not to rise in respectability and usefulness.

Another important measure adopted by the Society, to awaken the medical ambition of the country, and call forth individual efforts to excel, may be best made known by an extract from the minutes. It was resolved,

"That a premium of fifty dollars be offered for the best Essay which may be presented to the society, at the next annual meeting, on the "*use and abuse of Calomel*," and that a committee be appointed, whose duty it shall be to make public the above offer of this society, and to receive such essays as may be written on the above subject and forwarded to the society; which essays shall be received in such a manner as to secure secresy and save the feelings of those, whose essays may be rejected. The essay selected by said committee to be entitled to the above premium, and read before the next annual meeting.

"That the same committee, be ordered to procure a suitable medal, which, or its value in money, shall be paid to the successful competitor." * *

"The President appointed the following gentlemen as a committee on Prize essays &c. Doctors Felix Robertson, James Overton, John H. Kain, and James King, and, on motion, the name of the President James Roane, was added to said committee."

Such are some of the objects held in view by the Medical Society of Tennessee, and a brief outline of the plan which it pursues for the attainment of them. Of the manner in which the members perform their duties, we have a favourable specimen in Dr. Kain's oration.

That production possesses many of the qualities of an ex-

cellent discourse; and we shall mention but one defect in it. It is too short. The orator has not allowed himself sufficient scope for that amplification and richness, with which he might have strengthened and ornamented his address, and, by sinking it deeper into the feelings of his audience, perhaps done more to secure the object of it.

In the choice of his subject—"Medical Emulation"—we think him judicious; and he indicates correctly the end which that "emulation" should aim at—the attainment of "the approbation of the wise and good, and the reward of public confidence, for being useful to mankind." The topics of the discourse are well selected and ably handled; the tone is manly, and the manner animated; and the style perspicuous, unaffected, and vigorous. The entire performance is flattering to the author; the more so, from having in it something which clearly indicates, that, by cultivating his powers, he can write hereafter much better. He has a freshness of mind, which shows that its growth may be yet increased, and an elasticity, by which it may be expanded and made to fill a wider sphere. We trust therefore that the gifts of nature to him will not be unavailing; that he will not allow his talents to take rust through inaction; and that the Profession have not heard from him for the last time. A few extracts from the oration will best prove what we have said respecting it. We submit the following therefore to the reader.

"Again, we occupy new ground for the cultivation of the medical sciences. From the Alleghany to the Mississippi lies a region, unexplored by medical science.—The constitutions of our atmosphere, our medical topography, our medical plants, all require investigation, and open an ample field for the exercise of a laudable emulation among the members of this society. The habits of the people, their food, drink, and habitations are materially different from those of the eastern cities, where most of our medical writers have dwelt; and still more from those of European countries, whence we import so much of our knowledge. It is known to us all, that diseases exist in those countries, which are ascribed to local causes, and that we have among us the same diseases, but the same causes do not exist. Thus cholera infantum is attributed to impure air of crowded cities, Typhus to contagion; but here we meet with these diseases without their reputed causes. It will be found too that some diseases which are very prevalent in the Eastern States, as consumption, croup, scarlet fever, and puerperal fever, are comparatively rare with us. This observation pro-

perly elucidated may lead to a happy investigation of their causes and treatment, and thus may reflect from the west some of that light, which has dawned on us from the east."

Again;

"I beg leave to call the serious and united attention of this society to a subject intimately connected with the progress of medical science in the State of Tennessee, and on which, more than any thing else, depends, our usefulness and respectability. I wish that I could extend my voice so as to reach the ears, and gain the attention, of every thinking being in our land, that I might show them the deep, the vital concern of every individual in the subject of *post mortem* examinations, and the dissection of human bodies. Nor, till this subject is properly understood, and the prevailing prejudices removed from the minds of our fellow-citizens, will the healing art attain, among us, that perfection and usefulness, of which it is susceptible. Thousands and millions will continue to "die of medicable wounds," so long as those, who practice medicine, are denied the privilege of examining the seats and causes of disease. In vain do we boast of our pathological knowledge, when deprived of this, the only means of obtaining information concerning the ravages of disease. Books may give us the history of symptoms; we may learn that at the bedside of our patients, and may talk very learnedly about inflammation of the mucous coat of the stomach, hepatization of the lung, and *ramollisements* of the brain; but unless we have inspected the tissues in their morbid states, we know nothing about the matter. In vain do we display our bright and shining instruments, or boast of our skill in surgery, when we are only permitted to handle them on the living. Who could write well if he used the pen only four or five times a year? and how much more difficult to cut with an unaccustomed hand, upon muscle, cellular substance, and aponeurosis, close to important nerves and blood vessels, amidst flowing blood and to the music of cries and groans?" * * *

"In France, the custom of opening the body after death is almost universal. The consequence is, that the physicians of that country occupy the enviable station of teachers to the rest of the world. Their advances in knowledge have of late been so rapid as to require our utmost industry to keep up with them, and our utmost faith to believe what they unfold to us. But France possesses no other advantages over the United States for the cultivation of medical science, than that which has been named. Let us unite and combine our utmost efforts in this country to effect a revolution in the sentiments of the people on the subject of morbid anatomy. Let us use much prudence not to offend by the coarseness of our language, or to disgust by a vapid boasting or a shocking publicity. Let us by all means avoid an indecent exposure of the human body in its mutilated condition to the gaze of the public, or the stare of the curious. Let every thing be done for good; nothing for the indulgence of a wayward fancy."

We shall close our quotations with a paragraph peculiarly worthy of the attention of the Faculty.

"To attain and preserve eminence, it is necessary that the physician should be a constant student. In other professions men may occasionally arrive at a respectable standing by the force of strong powers of mind, without much aid from books. But the science of medicine is a continually increasing mass of facts, accumulated by the

labor of many generations. New observations and discoveries are continually making, and old errors are corrected; so that those who read the new publications are truly becoming "wiser than their teachers were." So extensive, diversified and complicated are the opinions and facts already published, that to know what has been written requires the labor of half a life. But no man should remain satisfied with the acquisitions, which may be made from books. His own observation should supply him with much information, and confirm or disprove the sentiments of authors. To enlighten and instruct himself, though honorable, should not be his sole aim: he should strive to add to the general stock of knowledge, and thus extend his usefulness to other countries, and future ages."

We have been long convinced, and the more we reflect on the subject, the more entire our conviction becomes, that the best plan that can be devised to improve the science and elevate the profession of medicine in our country, will be to *federalize* them; we mean, to place them under a general scheme of direction, assimilated as much as possible to the government of the nation. To speak more definitely. Let each state in the Union institute a Medical Society of its own, resembling in character that of Tennessee, and including all the respectable physicians within its limits. These societies, like the State Governments, will be independent of each other, and will transact, each within its own sphere, such medical business as may devolve on it. Their meetings will be annual, and held in the capitals of the several States. But let them also have a Diet or General Council, to meet triennially, or oftener if necessary, in the capital of the nation. This shall consist of members delegated from the several State Societies; and its business shall be to superintend the interests of the Profession throughout the Union.

Such is the outline (though brief, we hope intelligible) of the scheme we would recommend. If we mistake not, its simplicity would greatly facilitate its execution; and its grandeur would render it imposing and useful, far beyond any thing of inferior dimensions, and beyond any thing that has been heretofore brought to bear on the profession of medicine. It is not our intention to enter, at present, into any details, either as to the organization of the State Societies or the Diet, or respecting the practical duties of the members. Nor shall we specify the various channels, through which the predicted bene-

fits to the public would flow. Those topics would furnish valuable matter for a distinct essay. We repeat our conviction, however, that, if carried into effect, the scheme here referred to would give to the profession of medicine an elevation and influence, which it has never had, either in this or any other country. The improvement produced in it would be moral and social as well as intellectual. The lights of the profession would be both concentrated and diffused, in a manner and degree, that could not be effected in any other way. Medical feuds, which have been hitherto disgraceful, as well as injurious, if not entirely suppressed, would be greatly diminished; powers and influences, heretofore divided, would be united and strengthened, and, instead of acting individually and often in opposite directions, the members of the Profession would cooperate in masses to a common end. In this way, they would become, in a short time, one of the most powerful classes of men in the community; whereas, at present, they are one of the feeblest. Physicians are a much more numerous body, than either lawyers or divines; and they possess, individually, as much intellect and knowledge. But have they as much weight in society? No; nor a tenth part of it. Ten lawyers or ten clergymen exert more influence than one hundred physicians. The reason is plain. The two former act in concert and by system, while the latter act separately and unharmoniously. It is time that this state of things, so humiliating to medicine, were changed; and it can be readily done by the scheme recommended.

We shall close this article, by expressing a hope, that, from the example of Tennessee, or some other motive, each State of the Union will institute a Medical Society of its own. This we deem peculiarly incumbent on the State of Kentucky. She is the eldest State of the West, and therefore bound, in duty as well as pride, to set a salutary example to her younger sisters. Has she done so in the present case? Far from it. Every effort made by her to form a State Medical Society, or to pass a law for the regulation of the profession, has failed, either from the indolence and neglect of the friends of the measure,

or the activity of its enemies. Nor is the failure confined to the profession of medicine. It extends to science and learning of every description. *Kentucky is doing nothing to promote, in any way high intellectual cultivation. If her* ACTS *speak her sentiments, she is hostile to it.* This truth, however painful and mortifying it may be, should be forced into her ear by every tongue, and presented to her eye by every pen; and every thing friendly to her interest and honour ought to re-echo it, until she shall be constrained to listen, and remove the evil. For an evil it is of fearful magnitude. As the result of it, her course in mental improvement is *retrograde*, if not *actually*, at least *comparatively;* while that of the neighbouring communities is *progressive*. Instead of being, what she easily might be, one of the most literary and scientific States of the Union, she is rapidly becoming one of the least so. The fine genius of her sons is neglected, while that of the natives of other States is much better cultivated. The consequence is soon and easily told. She is fast losing her rank in the Union, and will never regain it, until she shall become true to herself, and foster institutions of science and letters. Excellent as the native intellect of Kentucky is, it cannot, uncultivated, cope successfully with intellects of an inferior order, enriched by study and strengthened by discipline. We would implore the members of her Legislature, then, by every consideration allied to patriotism and enjoining duty, to withdraw themselves, during some portion of every session, from the all-engrossing and ruinous theme of party politics, to deliberate on the mode and provide the means of promoting education among their constituents. It is such a course alone that will testify to their fidelity in their high and responsible station; nor can they, by any other, elevate Kentucky to the standing she should aim at, and secure to themselves the approbation of their contemporaries and the gratitude of posterity. C.

MISCELLANEOUS INTELLIGENCE.

Operation of Lithotomy by a native Doctor of Hindostan. (*Transactions of the Medical and Physical Society of Calcutta*, vol. iv.)—A short but interesting narrative is given in this work by Mr. Lindsay, of an operation for the extraction of a urinary calculus, the performance of which by a native of the Sutheya or surgeon caste, he witnessed in common with a medical friend. The man said he had operated on about 150 cases, and that not more than fifteen or sixteen died. He did not pretend to any thing in the way of operations beyond lithotomy, depressing the cataract, extracting nasal polypi, and treating wounds and sores. His mode of operating was by "cutting on the gripe," and his instruments constituted a genuine *apparatus minor*, for his whole stock consisted of an old lancet, a small folding knife, and a piece of unpolished iron about the size of a common uncut drawing pencil, having one end roughly scoop-shaped. Mr. Lindsay having ascertained by interrogating the man that he had really some knowledge of what he was going to attempt, resolved to patronize him by his presence. The patient was a boy seven years old, who had suffered dreadfully for several months, and in whom he himself, as well as his friend, easily detected the stone by examination through the rectum. Lachman Singh, the operator, without any preparation, and even allowing the boy to evacuate his bladder just before he commenced, proceeded in the following manner:—Having gladly availed himself of a double-edged scalpel which was offered him by his European friends instead of his clasp-knife, he placed the boy on his father's knees, brought each hand behind the ham, and directed them to be held firmly by the wrist, and the legs to be separated to a convenient distance. Then kneeling in front, he introduced two fingers of the left hand into the anus, and roughly pressed with the right above the pubes for a few seconds till he made the stone press out the skin to the left of the perinæal raphe. He now cut freely down upon the stone, making an incision in the same direction as in the European lateral operation, and proceeded to attempt the removal of the stone with his scoop. With this he failed at first; but on dilating the wound at the recommendation of Mr. Lindsay, he at once removed an oval calculus larger than a pigeon's egg, and weighing about half an ounce. The whole operation lasted only two minutes, and would have been over in a single minute had he made the incision large enough at first. The after-treatment consisted in the application of a variety of ointments and poultices, to which the operator attached great importance, but of which the principal recommendation in the narrator's opinion was that they did not contain a single ingredient calculated to do harm. Eight days after the operation the wound looked well, and the urine had begun to flow by the urethra: and three days after that every thing went on favourably.

Case of General Emphysema produced by Combustible Gas. (*Archives Generales de Medecine, Janvier* 1831.)—M. Bally lately presented the following extraordinary case to the Parisian Academy of Medicine. A man twenty-five years of age, was admitted into the Cochin Hospital at Paris in the fifteenth day of typhoid fever. He complained of violent pain in the left thigh, which, as well as the scrotum, was swelled; and in his delirium he talked of having been bitten in the knee by a dog; but no information to this effect could be procured after a diligent inquiry. He died on the day after admission; and in eight hours the body was examined.

Blood had issued from the nose, and from the surface of the skin of the thighs and head, where the cuticle had been stripped. The whole body was emphysematous, but especially the left leg. This was twice its natural size, had a brownish-violet colour, and was extensively covered with black and white phlyctænæ; and a reddish serosity, mixed with air bubbles, issued from the black ones. This limb resounded when struck, and crepitated when handled. The belly was much distended with gases. The face and temples were livid; and when the skin there was divided a great deal of reddish-black blood issued. The brain and lungs did not present any unnatural appearance; the heart was pale and empty; the intestines presented the usual organic derangements observed in typhus, (enlargement, induration, and ulceration of the glands of Peyer and Brunner.) Bubbles of air filled the vessels of the pia mater and left saphena vein. The lymphatic glands of the mesentery were enlarged, and contained a gas which took fire at a candle and exploded. The same phenomenon was witnessed after scarifications of the legs, thighs, and serotum. A puncture having been made in the belly, the gas which issued took fire also, and formed a flame blue at the base, white at the apex, and which burned for some time. The combustion likewise extended to the edge of the opening made with the trochar; and the edges became black and were consumed, so that the aperture was rendered of twice its previous diameter. The gas contained in the subcutaneous cellular tissue was inflammable, like that in other parts.

M. Bally considers that this evolution of inflammable gas was not a phenomenon which occurred after death only, and puts the question, whether the case throws any light on the spontaneous combustion of the human body?

Case of the successful treatment of Snake-bite by cupping-glasses. (*Transactions of the Medical and Physical Society of Calcutta*, Vol. iv.)—Mr. Clarke, assistant-surgeon in the Bengal service, has related the following instance of the successful application of Dr. Barry's treatment against the effects of poisons introduced into an external wound.

A fine healthy youth, fourteen years of age, was bitten at 10 P. M. near the bend of the right arm by a snake, the species of which could

not be ascertained. He was immediately conveyed to a set of "Snake-Doctors," who, after practising their quackery for five hours to no purpose, directed him to be taken to a European surgeon. Mr. Clarke, who saw him immediately after the native doctors gave him up, found him affected with stupor, insensibility, laborious breathing, extreme feebleness and rapidity of the pulse, great expression of anxiety and distress, constant efforts to expel frothy matter from his throat, and inability to articulate. The wound was situated a little below the bifurcation of the vein at the bend of the arm, extended horizontally half an inch under the skin, and a slight elevation was perceptible over its course, which was paler than the surrounding integuments, so as to present the appearance of a splinter of fir under the skin. The adjacent integuments to the distance of two or three inches were tense and painful. The wound was immediately scarified and a ligature applied above the elbow. At the same time two attempts were made to administer ammonia, but without success, as the muscles of deglutition were thrown into violent convulsive action, producing increased difficulty of breathing. Mr. Clarke then resolved to try the effects of cupping-glasses, and immediately applied one in the usual manner. After it had been applied for an hour and a half, with occasional short intervals, a manifest improvement had taken place. This was seven hours after the bite. He was more composed and expectorated less; the pulse was fuller, and the sense of suffocation less urgent. The cupping-glass was now removed, as very little blood had been drawn by it; and ammonia was administered from time to time, but at first with difficulty. In the course of another hour he could stand up, and soon afterwards he was able to walk. Twelve hours after the bite, the bandage having been still kept on the arm, he complained only of drowsiness and difficulty in articulating. In twenty hours the drowsiness continued, the eyes were dull and injected like a person recovering from intoxication, and there was indistinctness of vision. The arm, from which the ligature had been removed a short time before, was painful near the puncture, and on the seat of the ligature. Next day he was quite well, after a long and refreshing sleep.

[The above Miscellaneous Intelligence is all from the Edinburg Med. and Surg. Journal, No. CVIII.]

THE

TRANSYLVANIA JOURNAL

OF

MEDICINE

AND THE ASSOCIATE SCIENCES.

For October, November and December, 1831.

ORIGINAL COMMUNICATIONS.

ARTICLE I.—*Introductory Lecture on the advantages and pleasures of the study of Chemistry.* By LUNSFORD P. YANDELL, M. D.

(Delivered in the Chemical Laboratory of Transylvania University, on the 11th November, 1831, and published at the request of the Class.)

GENTLEMEN,

THE occasion on which I now appear before you, is to me one of strong emotion. But a few years having elapsed since I was myself a pupil in this University, it is with no affected diffidence that I now rise to claim your attention in the capacity of a teacher. If I may not dissemble the gratification which I feel at being thus honored, the solemn responsibility attached to the calling, be assured, is neither unknown to me, nor lightly regarded. I trust my deportment will not fail to testify that my sense of it is as deep as it is conscientious. I am fully aware, that while I hazard in it my own reputation, which is of more importance to me than life, my performance

of the duties, to which I have been invited, must have some effect, for good or for evil, not only on this institution, but in preparing you for the profession which you have chosen—a profession, not surpassed by any other, in its elevation and bearing on the welfare of society.

Since my appointment to the place where I now stand was first announced to me, these considerations have never been forgotten. But while they fill me with solicitude, they do not entirely bury me in despondence. They will only serve, on the contrary, to confirm my patience in research and preparation, and invigorate my efforts under a determination to do well. And limited as my experience and observation are, they have nevertheless taught me, that in most of the arduous enterprises of life, industry and perseverance accomplish more than genius and talent. And these, fortunately, are at the command of every one. As far, therefore, as they will go towards accomplishing a task which I regard as in the highest degree both difficult and responsible, I flatter myself I may be permitted to promise, without incurring the charge of presumption, that I shall not be found wanting.

Nor am I compelled to rest my hopes, in the performance of my duties, entirely on my own unaided efforts. On the labors of my colleague, of whose qualifications and accomplishments his presence here renders it improper that I should speak, I have much well-grounded reliance. You have a sufficient guaranty in his success as a teacher elsewhere, that he will not fail within these walls.

To these remarks I shall only add, that I do not feel at liberty to doubt the disposition on your part, to extend to me, in all cases, that degree of liberal indulgence which the enlightened and ingenuous never withhold.

And now permit me to proceed to the subject of my address. My duty, as you are aware, in the chair which I have the honor to occupy, is to give instruction in Chemistry; and I have concluded that I could not do better on the present occasion, than to present you with a brief view of the importance of that science, accompanied by a few of those beautiful, curious, and

interesting facts which have riveted the attention, and even excited the wonder of the civilized world.

That Chemistry, as a branch of scientific and collegiate education, has been greatly undervalued, not to say, seriously neglected, in the Western country, is a fact which, it is believed, will not be called in question. It would constitute a subject of curious inquiry how many thorough chemists we should be able to find in all our colleges and universities—averaging some three or four for each state, in all the older states of the west. How many who could analyze with accuracy, a medicinal spring, or detect the composition of a new mineral? Who, after examination, could pronounce positively, in a court of justice, upon the existence of a suspected poison? who, if required, could prepare for a physician prussic acid, or caustic potash, or sulphate of quinine? or who could determine the proportion of alloy in a counterfeit coin as was done by Archimedes for Hiero, more than two thousand years ago?

But, that Chemistry is a science, most important in its bearing, and of wide and varied relations, not only presenting to the mind of the student the most striking facts, and unfolding some of the most interesting laws of nature, but constituting for the professional man a field of knowledge as indispensable, as for the general scholar it is replete with attractions, is a truth universally received in other countries, and which will not long be questioned in ours. To be satisfied that medicine rests upon it, *as a chief corner stone*, let me entreat you to cast your eyes over the Materia Medica, and count the remedies which the physician has not received at the hands of the Chemist. Beginning with *Raymond Lully*, who discovered nitric acid in the 13th century, or with *Basil Valentine*, who, about the close of the 15th, introduced into practice, mercury, antimony and sulphuric acid, and coming down to our own times, you will find very few of high value, which may not be pointed to as trophies of the science which it is our object, in this address, to recommend. Those which it did not furnish, it has improved. It has given us accuracy—and in a most momentous concern—where every thing was once vague and uncertain;—and com-

parative elegance and comfort, where all before was loathsome and revolting.

For opium, which, after producing sleep, disturbs the system with subsequent fever and restlessness, it has given us morphine, which, as its name imports, induces a sleep at once tranquil and refreshing. In place of the ponderous, and offensive bark, it has substituted quinine, which the patient is able to swallow, almost without tasting it. By means of the oil of cantharidin, infused into a piece of paper, it is in our power to produce vesication, promptly and effectually, with the saving to the patient at the same time, of much of the pain of blistering. Iodine, an active remedy which has been applied successfully to the cure of a number of diseases, and whose remedial application, there is reason to believe, will yet be greatly extended, is a discovery of this science, which, 20 years ago, was unknown to the profession. The medical properties of sarsaparilla have lately been obtained in a form convenient to the physician, and less disagreeable to the patient. Piperine and strychnine are of comparatively recent discovery, and we are warranted in anticipating the most beneficial results from their extensive application to the treatment of diseases. And, from some late researches on the subject, there seems ground for the belief, that the principle which the Chemist extracts from *coffee*—the delightful morning and evening beverage of so many thousand persons—may be added, with advantage, to the already extended list of remedies for that most obstinate malady—*intermittent* fever. Chlorine is growing into extensive use as a *disinfecting agent.* The fate of too many new medicines ushered into the world with the most unmeasured eulogy, admonishes us to speak modestly of untried remedies. But if any reliance may be placed on public statements, it is not too much to hope, that chlorine may yet be made as effectual in destroying the contagion of small pox, and similar diseases, as it has proved, in the arts, in the removal of colors. *M. Labarraque* believes he has shewn "the possibility by means of it, of rendering the propagation of contagious or infectious diseases *impos-*

sible."* Time will not allow us to speak of salicine, prussic acid, phosphoric acid, and a number of other articles brought to light by the labors of modern Chemists, and which, it is hoped, may become invaluable resources to the physician, in the further improvement of the healing art.

If, therefore, in the early ages of medicine, Chemists were the discoverers of the most useful remedies, it is no less true, that the labors of these men are extending the materia medica

* Dr. Robert Christison, p. 703, remarks: "The success which has been obtained in the course of last summer to combat, to arrest, or to prevent the disease which attacked horse, a disease which may be considered as a pestilential epizootic or gangrenous typhus, immediately contagious, leads us to presume that the (the chlorurets) may be equally efficacious in the pestilential diseases of man, and with so much greater reason, since the experiments recently made by M. Laugier, surgeon in chief of the hospital La Pitie, in Paris, prove that the air of wards where small-pox patients are confined, no longer communicates the disease, whilst daily sprinklings with solutions of the chlorurets are employed."

"M. Labarraque communicated personally to the author (Alcock, on the use of chlorurets of oxide of sodium and of lime, p. 58,) that the infection arising from measles, which had occurred in a boarding school, had been perfectly arrested, without the removal of any of the pupils, this security from infection having been effected by the free use of the chlorurets."

The same author continues: "The beneficial results already obtained in arresting the ravages of infectious diseases, afford a well grounded presumption that the chlorurets may also be effectual in preventing the ravages of the plague, the yellow fever and other pestilential disorders."

The following interesting case of Asphyxia, successfully treated by M. Labarraque, and related in the 8th volume of the Archives, is quoted by the same author.

"A workman of a vermicelli maker was exposed to a deleterious gas which proceeded from an accumulation of filth and rubbish, heaped together from a pit of night-soil which was undergoing repair. He fell without consciousness. M. Labarraque was called to the patient soon after the accident; the symptoms were, pulse strong but fugitive on pressure, excessive rigidity of the limbs; arms stretched, stiff, and almost cold; head thrown backwards; veins of the neck turgid; face violet colored—also the lips, which were much swollen; eyes closed, dull and insensible; respiration appeared extinct; the danger seemed imminent. M. Labarraque details the train of reasoning which led him to adopt immediately the concentrated solution of the chloruret of oxide of sodium. A napkin moistened in the solution, was placed under the patient's nostrils, and in less than one minute he uttered an acute and plaintive cry or groan: the rigidity ceased; his eyes opened to shut again in a few seconds: the tetanic rigidity had re-appeared with its frightful train. I (M. L.) had withdrawn the chloruret too soon. The usual stimulants again tried, produced no effect. The chloruret was re-applied: in less than a minute the rigidity of the limbs ceased and the patient sent forth a piercing cry, which was stopped by the linen impregnated by the chloruret. A full inspiration took place, the air necessarily passing through the moistened linen, was therefore charged with chloruret saturated with water. The disinfection of the gas contained in the chest was no doubt complete, since the symptoms ceased: He was made to walk into the street, keeping the chloruret under his nostrils—his countenance regained its natural appearance, and he was soon in a condition to resume his work."

in our own day, and that Magendie, Dumas, Pelletier, Gay-Lussac, and others, are contributing a full share to the improvement of our profession.

Let me ask seriously, whether any physician, educated at this period, and in a University where all the branches of medicine are professed to be taught, can reconcile it to his feelings, to remain ignorant of the ingredients which enter into the composition of his remedies? It is not required that he should possess the skill to manufacture them. But can he be satisfied while he is unacquainted with the chemical difference between cream of tartar and tartar emetic, between calomel and corrosive sublimate, between oxalic and tartaric acid? Will any gentleman agree to take leave of his *alma mater*, invested with the honors of the doctorate, without an acquaintance with the antidotes to the many poisons, which, by accident at first, and through ignorance afterwards, are so often proving fatal to human life? Instances not unfrequently occur, where corrosive sublimate has been taken for calomel, or oxalic acid for epsom salts, or tartar emetic for cream of tartar; or, where these, and other poisons, have been administered with felonious intentions. What must be the mortification and the agony of the physician, who, summoned to render aid under circumstances so afflicting, and conscious that effectual remedies exist, is yet precluded by his ignorance from a resort to them? Whatever may have been heretofore the charity of the public, in the indulgent exercise of which, deficiencies so palpable and gross have been excused, we may rest assured that the period has arrived when such ignorance will meet with neither the connivance, nor the toleration of society. The march of improvement in the West is onward. The attainments of physicians must be extended also. The armor of the Faculty must be polished anew. It is the prerogative of the medical profession to stand on elevated ground. Its votaries are permitted, and it is their imperative duty, to follow nature into her inmost recesses, and witness the mystery and magnificence of her hidden operations. It holds connexion and claims support, from every other science; from mineralogy with its gems, and botany with

its flowers. And a calling of such concern is privileged thus to exact the aid of every department of knowledge. What does it not embrace, that is valuable in life? Not health alone, for if the mind stood firm, there might be something of life enjoyed without this. Not *bodily* strength alone, for this might be surrendered could we retain the vigor of our divine part. Nor the happiness of individuals alone; but the health, the happiness, the well-being of communities, of nations—the body, the mind, all are involved in the profession of medicine! To this profession, the palm of learning has been assigned by Dr. Parr, who was himself, incomparably the most learned man of his day; and by Jefferson, Dr. Johnson, Sully and others not less distinguished. Be it ours to maintain an ascendency so honorable.

A task somewhat peculiar belongs to the medical profession of the West, in the execution of which, a knowledge of chemistry becomes indispensable. The physical character of our extended valley has yet to be illustrated. Its geological formations have not been explored; the character, locality and richness of its minerals have not been ascertained, and especially, its mineral waters remain to be analyzed. Here is a harvest just ripening for the sickle, with scarcely a laborer to divide with you its honors or its rewards. Much, we have reason to be assured, exists in the structure of our rocks, to instruct and delight the Naturalist; mineral treasures doubtless abound, which when developed, will enrich communities as well as individuals; and medicinal springs are scattered all over the country, whose waters have proved effectual where medicine had failed. They all pass under the general vague names of *sulphur* and *chalybeate*, and nothing is known of them further, save that dyspeptics, and hypochondriacs, and other valetudinarians have resorted to them and been healed.

These springs constitute a portion of the wealth of every country. Besides the recreation and luxury of visiting them during the heats of summer, many of them possess decided efficacy in numerous chronic complaints. Those containing iron are known to be adapted to cases of debility; but they

produce mischief where inflammation exists. To one class of patients, those containing magnesia, and other aperient articles might be well adapted; and yet they would scarcely fail to aggravate the complaints of another class who suffered from extreme debility. But how are the sick to make these discriminations, when their medical advisers are as blind as themselves? And *experience*, in such cases, might prove too dear a teacher.

It will come to be regarded as a stigma upon the West, if the investigation of these sources of health and comfort is much longer neglected. The only ground upon which past dereliction has been excused, is our infancy. While every other quarter of the globe is pouring its treasures into the storehouse of science, will it not cause the cheek of patriotic pride to mantle with shame, that our great valley, rich as it is in minerals, rich in animal and vegetable productions, and not less rich, let me add, in native intellect, should alone withhold its contributions? The long cheerless night which brooded over it during the generations of the race which has just disappeared, only *begin to give way*, with the coming of their successors. The grey of the morning, as regards its science, still hangs around it, and we are just beginning to anticipate a brighter day. Let me indulge a hope, that I behold before me, at this time, the ardent, intrepid corps, before whose labors, the last lingerings of this intellectual night will finally disappear. We cannot be mere physicians. We must be members, (we ought to be cultivated, variously taught, armed with knowledge at all points) of a great, busy community, of multiform interests and vocations. With all these chemistry is connected; to all it lends its light to instruct, to gratify and improve.

In many processes of domestic economy, a knowledge of the principles of the science is of signal advantage. From the baking of bread to the construction of our fire-places, from the regulation of our apparel to the erection of metallic rods for the defence of our dwellings, these principles are put in requisition.

The lime-maker who exposes his rocks to an intense fire, for

the purpose of driving off the carbonic acid, of which limestone is in part composed, is as much the Chemist, perhaps without suspecting it, as he who obtains oxygen by a similar process, from manganese, or nitre, in his laboratory. So the house-keeper who adds quick-lime to lye for the purpose of giving it strength, and oil afterwards, to convert it into soap, avails herself of chemical affinities, how little soever she may understand of their nature. And the farmer, who spreads lime, or plaster of Paris over his fields, and the black-smith with his bellows, the forger of iron, the assayer of silver, the tanner, and the manufacturer of steel, gun-powder, glass and porcelain, are all chemists, it may be, without an acquaintance with the principles of the science.

Chemistry is at the foundation not only of these, but almost every other art. It assists the brewer, and the maker of wine. The dyer of stuffs is aided by it, and the bleacher of cloths, taking advantage of the instruction which it affords, accomplishes now in weeks, what was once the work of months. The refining of sugar is a process conducted upon principles strictly chemical, and an improvement made in it, by an accomplished Chemist, a few years since, was the means of enriching, with slight expense, and almost without trouble, most of the persons engaged in the business. The safety lamp of Sir Humphrey Davy, which has been truly called "the most beautiful and useful invention of modern times," and which is a glorious triumph of the science over destruction, was made by one whose life was devoted to its cultivation. Until the time of this invention hundreds of miners were sacrificed, in their useful calling, by the explosion of carburetted hydrogen gas. Dr. Wollaston had observed, that this gas could not be kindled in a glass tube so narrow as one seventh of an inch in diameter; and profiting by the discovery, Sir Humphrey constructed this invaluable instrument, which, while it guides the miner in his labors, shields him effectually from all danger. Finally, the steam engine, which performs now half the work of society, the conquests of which have already surpassed what fancy had conceived, and whose future conquests promise to extend further

than imagination can reach, is a chemical instrument, and owes its discovery to a philosopher who thoroughly understood the principles of the science.

Nor, we are permitted to believe, has Chemistry yet conferred upon the arts all the benefits of which it is capable. Even while we are here discussing the subject, the public journals are bringing us accounts of the manufacture of an illuminating gas *from water*. The hydrogen which the water on decomposition affords, is passed, according to the account, through a liquid obtained during the distillation of pit-coal, which causes it afterwards to give out a bright light in combustion. Thus, it is playfully remarked, "the exploit of 'setting the great river on fire,' which our ancestors, in the simplicity of their hearts, reckoned among the number of impossibilities, is now in a fair way of being performed. The cold waters drawn from the bosom of the earth, are to be distributed in currents of flame, from habitation to habitation, and the element which we use to quench fires, is to light up our cities, with a brilliancy scarcely to be surpassed by that of a conflagration. When the anthracite mines of the country shall be exhausted, and our forests all hewn away, our posterity will be able to keep themselves warm by burning the lakes and rivers."

Why is it that the tradesmen of Great Britain, as has been truly remarked, "can furnish at this day, more profound thinkers on philosophical subjects, more acute and accurate experimenters, more real philosophers, thrice told, than all Europe could furnish a century ago?" It is because Chemistry and the physical sciences have attracted there the attention due them.

If the same encomium pronounced upon the tradesmen of America, would sound, at this time, like "satire in disguise," I am not sure, since the opportunities afforded them, by Lyceums and other popular literary institutions, for acquiring a knowledge of these sciences, are so much increased, that it will long be so. And hence the members of our learned and liberal profession will be forced to extend their knowledge in this direction, or see themselves excelled by men of much humbler pretensions.

Another inducement to the prosecution of this study: The great mass of the people of our country live not only by the sweat of the brow, but by the tillage of the soil. Among this people, you are going when you have completed your labors here, to fix your homes. What are their interests must become yours, as they are those of all classes of the community. When they prosper, every other trade, and profession must prosper. What more agreeable task, then, could you desire, than to instruct this most useful class, in the application of *Chemistry* to *agriculture;* which, while it enriches *them*, will benefit yourselves? Glauber labored to make this application, while Van Helmont was shewing the connexion between medicine and chemistry, nearly two centuries ago, and it is singular that whilst all his other discoveries have been turned to good account, this the most important of them, should have been so lightly regarded. Sir Humphrey Davy, of whom it is as true as of the individual to whom the remark was at first applied, that "he touched nothing which he did not adorn," rendered the study of Chemistry in connexion with agriculture fashionable in England by his lectures on that subject. But the influence cannot be said to have reached this country. Be the intelligence of our agriculturists on other subjects what it may, (and no one has a higher respect for it than the author of this address,) it is useless to deny, that upon this, there prevails among them a lamentable degree of ignorance. Of the *science* of agriculture, it may indeed be said, that they know almost nothing. Lime, plaster of Paris, and occasionally burnt clay, are spread upon our fields, but their *modus operandi* is as thoroughly a secret, as if the whole process were an act of incantation. Whether the soil is argillaceous or calcareous, magnesian or silicious, whether vegetable matter is deficient or in excess, are questions, which, however necessary to be determined, when an attempt is about making to increase its fertility, are such as the science of a whole neighbourhood would not be sufficient to answer.

It is, hence, in your power to become (and I trust the classes of this University *will* become, by promoting the study of che-

mistry, by getting up Lyceums, and in lectures, pointing out the improvements which chemistry is capable of making in it) the efficient patrons of agriculture, than which, Cicero has said, "no calling is more dignified or delightful, or more becoming a free man." And, as the bread cast upon the waters was gathered up after many days, you will have the gratification of seeing a community increasing in knowledge, and growing in prosperity by means of the light disseminated by your labors.

If then, you would form an adequate conception of the range and importance of chemistry, you must look back over the arts which it has invented and improved, and of which but a part have been enumerated. And it will not fail to be perceived, that of the principles of all these, he must remain ignorant, who is unacquainted with chemical science. So much of the volume of nature, he will find himself unable to interpret. The fire that blazes cheerfully on his hearth, will not be more intelligible to him, than the fire that bursts from the bowels of the volcano. The rain that falls, the wind that blows, the freezing of water, and a thousand such familiar occurrences, must present to his mind a perpetual mystery.

Such is the value of this science. But, apart from its utility chemistry opens a field of investigation, in which the mind is delighted with the discovery of new and striking truths. And this is a pure, elevated, and enduring pleasure, a pleasure known to every student of nature, and which has this advantage over most others; that it is exhaustless, terminating only with the discovery of all truth; that it is purely intellectual, and consequently can never pall; that it is wholly without alloy, since it carries with it the approbation of the conscience. Such was the emotion of Sir Isaac Newton, on approaching one of the most important of his discoveries, that it is said, he was compelled to give up the calculation in which he was engaged to be completed by a friend. And Archimedes is said to have been so transported by the discovery of the means of determining specific gravity, that he ran from the bath, in which he

happened to be at the time, proclaiming it through the streets of Syracuse.

If a traveller should propose to lead you to a new country, whose scenery had no likeness in any thing with which you were acquainted, whose animals were of new species, and of great value, which abounded in beautiful and precious gems, and whose surface smiled with rare, and useful plants and flowers, need I say that you would be impatient to visit it? And, if it were added, that its position was remote, and that the road to it was steep, and difficult of ascent, would this daunt your manly spirits which say, that "the joys of conquest are the joys of a man?"

But the Chemist proposes to open to your view a world, upon the like of which the eyes of the uninitiated never rested, which, lying below the surface of things, yet presents laws and transformations having nothing more remarkable in the descriptions of fairy-land.

What, for example, is better calculated to strike us with surprise, than that the sun-beam, which, to all appearance, is one of the most simple of substances, and which is so light that it does not affect the most delicate balance, should yet be divisible into seven prismatic colors, and that besides these, which occasion light, it contains other colorless rays which produce heat, others again which separate oxygen from its compounds, and yet others, which render iron magnetic; or that, from the combination of these seven colors, red, orange, yellow, green, blue, indigo and violet, *white* should result, which is generally esteemed *no color?* Or that this simple sun-beam, variously reflected, should give rise to all the hues which we meet with in creation; as well that of the verdant mead, as that which adorns the pigeon's neck, and that which delights us in the azure vault, or the rain-bow, and even that which we admire in the blooming cheek of beauty?

But this is not more remarkable than the fact, that a number of zinc and copper plates, alternately arranged, in the form of a Galvanic battery, should be capable of exciting a degree of heat sufficient to burn gold, and to melt platinum, which resists

the heat of the strongest furnace, as wax melts before the fire.

The student of this science meets, perhaps, with nothing more admirable, than that phenomena so varied and so numerous, should result from the operation of a few simple laws; and that from elements so limited in number, there should arise such endless variety in the modifications of matter. The combination of oxygen with a combustible, is the source of more than half the changes going on in the world around us. It is this operation which we witness in the combustion of our wood and candles. It is this which we observe in the rusting of metals, in the formation of nearly all the acids, in the conversion of many solids into gases, and in the respiration of animals. It is the same which we see in the wonderful phenomena of the *compound blow-pipe*, where the union of hydrogen and oxygen is attended with the most intense heat, and where the result of the combustion is water. It is also the same principle in operation in the brilliant experiments with the *Deflagrator*, in which metals and charcoal are consumed with an intensity of light, scarcely surpassed by that of the mid-day sun. And yet what processes are more unlike each other than the rusting of metals, the burning of wood or candles, the production of acids, and our own respiration?

The fruitfulness of nature in producing variety out of a few elements is exemplified by *carbon*, in its various forms and combinations. This substance composes, in a good degree, the fuel which we consume, whether in the form of wood or coal, the food upon which we subsist, the clothing which we wear, and the materials for artificial light. It constitutes, chiefly, the feathers of birds, the hair of quadrupeds, the scales of fish and reptiles, and the fat of all animals. In combination with oxygen, it enters largely into the composition of our limestone rocks, is the sparkling ingredient of soda-water, beer, cider, and Champaign wine, abounds in many mineral springs, and is found in nearly all our spring and well water, for without it, even this "pure antediluvian beverage," would be comparatively vapid. With oxygen, it forms an aerial acid. With hydrogen, an inflammable gas. In one form it appears as char-

coal, in another as the brilliant diamond, and in a third, as plumbago or black lead. Vegetables mainly subsist upon it; and, in combination with oxygen, hydrogen and azote, it is the largest ingredient in the composition of our own bodies. In the shape of a gas, it is escaping continually from the lungs of animals, and from decomposing vegetable and animal matter. It is computed that 11 ounces troy, are disengaged from the blood every 24 hours; which amounts to more than twice the weight of a living man in the course of a year. In *one* shape it is one of the most friendly agents which man has under his control, contributing liberally to his enjoyment, and even sustaining his life. In another, it is the reverse. Its action upon the lungs in the form of an oxide or acid, is eminently poisonous, producing death in a few moments, as happens to animals exposed to the *Grotto Del Cani*, near Naples, and as has befallen many an unwary laborer, from descending into old wells. A substance thus deleterious and so extensively generated, would become, in a short time, a source of much inconvenience, and even lead to the extinction of animal beings, were not ample provision made in nature for its removal. But here, as in all other cases, we witness that perfect system of checks and balances, by which all things are kept within their proper sphere, and harmony and order are made to result. Water has a strong affinity for it, and its base is the food of plants, and hence it is no sooner disengaged, than our seas, lakes and running streams are greedy to devour it, and myriads of vegetable mouths receive it as their fit aliment.

In this manner, the vegetable kingdom, aided by collections of water, performs the office of a great depurator of the atmosphere, which is becoming constantly defiled by the contact of animals. Thus, by a wonderful agency, we see poisons converted into healthy food; and that out of a single element, variously modified and combined, are formed the clothes which adorn our persons, the fires which warm us in winter, the light which cheers the gloom of our long nights, and much of the aliment by which we are sustained.

We see, moreover, in these changes, an admirable provision

made for the growth, decay, and re-production of animals and vegetables. The carbon which had become offensive to animals is expelled in respiration, enters into the composition of plants and contributes to their growth, and comes in circling years again to be the food of animals. So too, when animals die, and are resolved into their primary elements—when, in other language, *dust returns to dust*, these elements, mingling with the soil, or floating in the atmosphere, are again imbibed by plants and made subservient to their wants: The two kingdoms seeming thus engaged in the practice of mutual reprisals; or, in another view of the subject, to constitute a circle of harmonious changes. In view of this simple and beautiful round of action, well may the student of nature exclaim with the poet,

> "Look round the world! behold the chain of love
> Combining all below, and all above.
> See plastic nature working to this end;
> Atoms to atoms—clods to crystals tend.
> See dying vegetables life sustain;
> See life dissolving, vegetate again.—
> All served, all serving, nothing stands alone,
> The chain holds on, and where it ends unknown."

Chemistry has, what are termed, its miracles and paradoxes. Gases become solids by uniting; and liquids, in a particular state, and under particular circumstances, are made to boil, by *the application of ice* to the vessels containing them.

Again, if four parts of sulphuric acid are added to one of ice, a liquid results having a temperature as high as that of boiling water; but if the proportions are reversed, and four parts of ice are added to one of acid, they produce a mixture intensely cold. In the first instance, the heat taken from the acid to dissolve the ice is much more than compensated by the contraction which takes place in the resulting liquid; but in the second the quantity of ice being so great, makes in melting, a draught upon the caloric of the acid, which there is not a reduction in the bulk of the liquid sufficient to compensate.

It is well calculated to excite our astonishment, that many of the bodies around us which appear so inert, should consist of ingredients of such wonderful activity. It has been already

mentioned that our lime-stone rocks are composed in part of carbonic acid, which when taken into the lungs is a deadly poison. Plaster of Paris, which has also lime for its basis, contains as another ingredient sulphuric, which is known to be a corrosive acid.

Fluor or Derbyshire Spar, known as the material out of which vases and mantle ornaments are fabricated, in addition to lime, consists of fluoric acid; which is so corrosive, that its vapours produce serious consequences when inhaled, and a few drops of it applied to the skin of an animal have been known to destroy it.

The bones which have been whitening in the sun and rain for half a century, and which seem as inert as clay or stone, yet contain a virulent acid, which again is composed of oxygen and a substance so inflammable, that it wastes away by slow combustion in the atmosphere, and takes fire at a heat but little above that of the animal body. And what seems yet more remarkable, prussic acid abounds in the kernels of many fruits, and is even formed during the decomposition of blood; and it might therefore be said that our bodies contain the elements of an acid, a few drops of which would prove fatal to our lives. We are thus led to remark, that we have been all our lives walking in the midst of the most explosive gases, the most noxious vapors and the most corrosive acids, the existence of which, tied down and controlled as they are by powerful affinities, we had never suspected.

Properties occasionally the most unexpected and surprising, result from the combination of the most simple substances; as where nitrogen and hydrogen, themselves perfectly inodorous, uniting produce ammonia, a pungent gas which constitutes the odorous principle of the smelling salt, hartshorn &c. And carbonate of ammonia, itself a solid, results from the combination of two permanent gases. While fluoric acid, a liquid, and silex, a substance so hard as to scratch glass, and which forms the foundation of the everlasting hills, on being united are converted into a gas.

In one case, we have the rarest of substances, hydrogen be-

ing nearly fifteen times lighter than the atmosphere, forming a solid; while in the other we are presented with the fact of a fluid, and an earth, but little inferior to the diamond in hardness, taking to themselves wings as it were, and dwelling in the form of perpetual vapor.

Such wonder-exciting results spring, not only from the union of *different* substances, but are presented by the same subtances united in different *proportions*. It is not only true that oxygen in union with carbon, is a very different substance from oxygen united to nitrogen or hydrogen; but that one proportion of oxygen united to one of these substances, is a very different compound from two of oxygen to one of them. If we take one measure of alcohol and dilute it with one of water, the properties of the alcohol though diminished are not subverted. And if another measure of water be added, the alcohol is still farther diluted, but its properties remain unchanged. Not so where the action is more decidedly chemical. One proportion of chlorine united to one of mercury, produces calomel, a mild medicine; but *two* proportions of chlorine to one of mercury, give a very different substance, corrosive sublimate, which, as its name implies, is a poison. Oxygen and carbon united in one proportion, give rise to a gas, carbonic oxide, which is lighter than the atmosphere, inflammable, and when inhaled, poisonous. In a different proportion the quantity of oxygen being doubled, another gas results having all the properties of an acid, and which has already been mentioned as an agent at once of wide diffusion and controlling energy. While in a third, the oxygen being intermediate in quantity between that of the other two, they give origin to a fixed vegetable acid, which is as unfriendly to the stomach as are the gases to the lungs.

But perhaps a more striking example is afforded by the combinations of oxygen and azote. In this case, six substances differing from each other widely in character, result from the combination of the self-same elements in different proportions. In the proportion of four volumes of nitrogen to one of oxygen they produce the atmospheric air.* In that of one of each

they give rise to nitrous oxide, or the exhilarating gas; in that of two of oxygen to one of nitrogen, the nitric oxide, a gas so hostile to life that warm-blooded animals immersed in it are killed almost instantly; in that of three of oxygen to one of nitrogen, to an acid called hyponitious: in that of four of oxygen to one of nitrogen, to nitrous acid; and in that of five of oxygen to one of nitrogen, the nitric acid, or *aqua fortis* of commerce. Into all these compounds the same elements enter; and they are the same which, since the hour of our birth, we have been receiving into our lungs. Chemists have said that nitric acid is occasionally formed in the atmosphere (from its oxygen and nitrogen) by electrical agency, and that it is detected in rain-water after a thunder storm.

Can any thing be conceived more truly wonderful, than that while *one* volume of *oxygen* and *four* of *nitrogen* produce the bland, invisible, impalpable atmosphere in which plants and animals live, and without which there could be neither light nor animal or vegetable existence, *five volumes* of oxygen and *one* of nitrogen constitute an agent which dissolves metals, sets charcoal on fire, and corrodes and destroys the animal fibre almost as soon as applied to it? And must not that be esteemed a fascinating study in which truths so remarkable, and in such endless variety are presented to the mind?

Chemistry moreover, rests upon a mathematical basis. Its truths are the result of observation, and are palpable and indubitable. The mind reposes upon them with the assured confidence of demonstration. It has placed the fact beyond question, that bodies, in most cases, unite in *definite proportions*—a sublime truth, which, as has been well remarked, "deserves to be inscribed on the same tablet with the laws of gravitation and projection;" and the chemist is now able to rectify or confirm the result of his analysis by an arithmetical calculation. Even more. His knowledge of this law has enabled him in some cases to precede experiment, and like Columbus in his discovery of America, to perceive the existence of compounds before analysis had brought them to light.

Of no science is the remark of Aristotle more true, that "ex-

perience is the basis of knowledge." The exhausting, unprofitable labors of the alchemists for century after century, prove how unavailing is conjecture in a science where "the way to truth lies only through the safe places of observation." Experiment tests the truth of every thing in chemistry; and its responses can neither be rejected nor gainsaid. When it has been shown, for example, that *table salt* according to the ancient definition of the term, is *no salt*, but *chlorine*, united to a *metal;* and that the base of potash is likewise a metal, as bright as quicksilver, and lighter than water, which burns brilliantly on ice, however paradoxical, we are obliged to assent to its truth.

It would seem well then, that the students of a profession, where there prevails so much uncertainty as it must be admitted still hangs over medicine, should be familiar with some ground over which they may move with a firm step. Having long doubted on the subject of the circulation of the blood, and waded through the interminable controversy concerning the contagiousness of yellow fever, cholera and typhus; and debated the whole ground of the treatment of fever, with the doctrines of Armstrong, Broussais, &c.; and followed *Hahnemann* through his learned but startling *homœopathy*, in which he teaches that calomel and blue pill, instead of being given as we give them in the south, by the *drachm*, are to be administered in doses of a millionth, or a decillionth part of a grain; having, I repeat, thus long wandered and doubted, it may prove salutary to bring the mind back again to matters of demonstration. He was an experienced and not an injudicious teacher in our profession, who required of his pupils as a part of their course, the study of Euclid's Elements. Chemistry may be made to the student of medicine, a sort of mathematical palæstra for the discipline of his mind.

In what study indeed, can the student of medicine so well engage, when not employed upon the immediate subjects of his profession. Medicine we have seen leans upon it as an important pillar. But this apart, would any of you engage in party politics? A thousand presses, and ten thousand tongues are already busily plying this task. Like one of our swollen

streams, they are sweeping over the surface of our whole country, and bearing every thing upon their turbid, tumultuous, it might be added, polluting wave. From a scene so full of angry and perturbing strife, all experience warns the lovers of science, and of philosophical repose, to keep aloof.

Do poetry and eloquence allure you? These truly are highly tempting paths; and there is an eloquence which every one may well cultivate; the eloquence of the fire-side, of the social circle, the fascinating eloquence of our countryman Franklin.

But poetry wanders far from the path of the physician. True, Darwin was a poet and a physician, and excelled in both arts. And Akenside wrote on health, as well as on the 'pleasures of the imagination.' But it is nevertheless true, that the physician has but little time to bestow upon the Muses, and the poet generally has little taste for the sober walks of philosophy.

Would you embark on the ocean of metaphysics? Every physician should be acquainted with the philosophy of the human mind. *Mens sana in corpore sano* can scarcely be preserved or restored without it. But if you will cast your eyes over the history of literature, you will find little to entice you to the extensive prosecution of this study, unless, indeed, you are tempted by speculations which have no end; by inquiries beginning in darkness and terminating in disappointment; by protracted labor which has for its reward nothing but doubt and disquietude. You will see that the fame of the first metaphysicians of their age is beginning to wane; that the stars of Malbranche and Kant, of Locke and Descartes, and even of Stewart, and Reid, and Brown, notwithstanding their lives of laborious study, are beginning to recede from their place of pride. Need any of us hope for a kinder fate? Will any one of the present day bring to the task, more genius, or greater eloquence, or profounder learning, or a purer love of truth?

Chemistry and its sister sciences, Mineralogy, Geology, Zoology and Botany, are peculiarly congenial to your professional pursuits. Most other studies require solitude and seclusion;

these may be pursued, and some of them are pursued to the best advantage, in the open fields. And the physician, as he finds his way through the country to the dwellings of his patients, may enjoy a perpetual feast in the ever-varying scene before him. Every day presents him with some new object, some novel form of vegetable life, or some new feature in the mineral kingdom. Whether able or not to find "books in the running brooks, sermons in stones, and good in every thing," brooks and trees and stones must often be his sole companions. Then "*felix qui potuit rerum cognoscere causas;*" happy is he who may discern the nature of the things before him! To the uninitiated every thing is as a sealed book. The stones hold no converse with him; the brooks are silent. He has no names for the myriads of animal beings presented to his eye, and the countless multitude of plants involve him in a perplexing labyrinth.

We live in a practical world, and in a highly practical age. The *greatest happiness principle* is the all-engrossing one, with rulers, people and philosophers. He who would enjoy consideration while he lives, or exist in the memory of future ages, must study to promote "the greatest possible happiness, of the greatest number, for the greatest length of time." A reputation based on any thing less than this, will in the very nature of things be short-lived.

The science which we are recommending, and which it has become our duty to teach, will make you *utilitarians* in the true sense of the word. Along this road, some of the first minds that adorn the annals of history have travelled to fame and immortality. Who, in fact of modern men have been of most repute? Who have filled the largest space in the worlds eye? We speak not of poets, for it is in our nature to prefer pleasure to instruction, amusement to business; nor of statesmen, for they control the affairs which come home at once to the bosoms of men; nor of warriors, for like the comet, "shaking from its horrid hair, pestilence and war," they are sure to attract the gaze of the multitude, whose lives and rights they trample on; but of men who have traversed the orbit in which you expect

to move, who have pursued science as their calling. Have any risen higher than our own Franklin, or Watt, or Count Rumford, or Sir Humphry Davy, or Newton, or Boyle, of whom we speak as of a higher order of beings? The name of Watt will float on the breath of fame while the "unconquered arm" of steam continues to widen its dominions, or history to record the achievements of genius. Count Rumford's was a life devoted steadily to the advancement of science, and the promotion of human happiness. His discoveries in chemistry placed him in the rank of the first philosophers of his day. But he deemed it an object not less worthy of his labors, to improve the construction of fire-places, to ascertain the comparative wholesomeness and value of different articles of food, and to compound soups, and devise means for feeding the poor, than to investigate the laws of caloric. And it is his glory, that he wiped away the reproach of beggary from Munich, the town in which he lived, greatly improved the condition of the indigent, won the entire confidence and favor of his enlightened sovereign, and obtained a rank among the benefactors of the human race.

Our countryman Franklin is another bright example of the advantages of a life spent in the cultivation of a *practical philosophy*. He labored to discover truth, and to apply it to some useful end. Like Count Rumford he applied his faculties to the improvement of our fire-places; and no sooner had he established the identity of electricity and lightening, and observed that they were alike attracted by points, than he set about devising means for protecting our habitations against its destructive force. He was a contributor alike to chemistry, natural philosophy and the science of government. And next to that of Washington, of all our patriots and philosophers, his is the most honored name.

Sir Humphrey Davy possessed a mind of the boldest and most original character; a quickness almost of intuition to discern the laws of nature, and patience and acuteness to test by experiment the accuracy of his theories. No one of the present age contributed so much to the improvement of chem-

ical science, or gave so many proofs of the application of that science to the practical concerns of life. And as evidence of the high estimation in which he was held in the great metropolis where he lived and labored, it may be mentioned that he was *unanimously* elected President of the Royal Society, succeeding to Sir Joseph Banks, and to the chair which had once been occupied by Sir Isaac Newton.

Let one example farther, drawn from the records of English biography, show how much richer in benefits is the cultivation of *physical* than *metaphysical* science.

Two greater philosophers than Locke and Newton never adorned the history of any country, or of any age. It was a remark of Aristotle that "there was a foolish corner even in the wise man's brain;" and yet, if there ever appeared on earth a man possessed of reason in its highest perfection it was the author of the Essay on the Human Understanding. He abjured the quiescent feelings only so far as was necessary to give him firmness of purpose, boldness of design, and energy of execution. He had no feeling or passion in excess. In his most animated controversies, where he was sometimes harshly assailed, he maintained a calm, equable, philosophic temper. During his unjust banishment, he was busy in composing works for the benefit of his country. His was a long life, sedulously devoted to science, and to the interests of his fellow-men.

Newton, though like Locke, an eminent philosopher, was not without his weak points of character. The current story of his lap-dog and papers is a fiction. That catastrophe is said to have made an impression upon his feelings from which he never entirely recovered; and it is certain, that although he lived forty-five years afterwards, he added nothing during all that time to his former brilliant discoveries. In his correspondence we hear him indulging in unjust complaints of his best friends, and see the display of a groundless jealousy, which warrants the suspicion that his great mind was obscured by a partial insanity during the evening of his life.

Such were the men living. Locke's writings conferred upon him universal popularity whilst he lived, and in his death, he

was abundantly honored. His Essay on the Human Understanding was regarded as the most original and profound work of its day, and superseded all others on that subject.

But we have said that his reputation is already on the wane. His great work carries in it the seeds of a fatal disease. Its subject is a science, which, from the very nature of it, must remain liable to frequent changes, and which affords, therefore, no basis for a lasting reputation.

How fares it with the author of the *Principia?* He toiled in a field whose products are as useful as they are brilliant and sublime. His discoveries were alike unquestionable, as to their truth, and unequivocal in value. His fame, like the house of the wise man in the parable, is based on a rock, and the tide of time which bears down every thing but truth, has beaten upon it in vain, and the discoveries of subsequent philosophers have served only to strengthen it.

In a word, nothing which is not *useful* deserves the attention of a wise man. Let this be the motto of every student of medicine. There is much for us to do worthy of our highest, most persevering efforts, much required by our duty to ourselves, our country, our God. The shortness of human life affords no time for trifling. The faculties of the most vigorous grow weary with incessant toil, in spite of their ambition, and the time of the most industrious must often be engrossed by unimportant cares. How necessary then, that we should direct, while able, all our energies to objects which are worthy of us, knowing that perplexing and distracting cares must come, and that the night of languor, of weariness and disease, as well as of the grave, hasteneth on, in which we can do no work!

Finally, you have seen that Chemistry, along with the other branches of Natural History, having been neglected in the west, you will have the gratification of laboring in a field possessing all the charms of novelty—that a feast will be supplied continually to the mind by the presentation of new and striking phenomena in nature, evidencing every where consummate wisdom and skill in the Architect—that the study will give strength and accuracy to the powers of your minds—that as a

branch of science, it has *close and important connexions with medicine;* and finally, that by the arduous pursuit of it, you may attain to eminence as scholars and philosophers.

The prospect before you is inviting, and you are allowed to indulge exhilarating hopes. There are many medals in the Natural History of the West which the industrious of you will receive; contributions to be made to the stock of science which will place the aspiring of you on the same catalogue with Linnæus, Hauy, Priestley, Lavoisier and Davy.

And let me say to you, that that individual who shall write the Natural History of the western country, present the locality of its minerals, describe its geology, the character of its animal and vegetable productions, analize its mineral waters and describe its diseases, with the causes which give rise to, and the method of preventing and curing them, if he may not be promised the boon of immortality, will, yet, both add to the glory of his country, derive a rich present reward of fame and money, and be ranked by posterity among the effective contributors to science.

ART. II.—*Address delivered before the Legislature of Tennessee, at Nashville, October 19th, 1831, by* GERARD TROOST, M. D. *Professor of Chemistry, Mineralogy and Geology at the University of Nashville; former Professor of Mineralogy at the Museum of Natural History, and Lecturer on Chemistry at the Apothecary Hall, at Philadelphia.—Member of the Philosophical Society—of the Academy of Natural Sciences, and Honorary Member of the M'Clurean Lyceum in Philadelphia—of the Medical, Chirurgical and Pharmaceutical Association of Holland; former Officer of Health of the first class in the army of Holland; Corresponding Member of the Royal Museum of Natural History in Paris; Member of the Mathematical Society and of the Society of Natural Science and Literature of the Hague; Corresponding Member of the Physical Association of Baltimore, etc. etc.*

[We transfer the following address to our pages from the Nashville Banner, for the purpose of giving it still wider circulation, and to preserve in a more accessible form so able an exposition of the physical resources of a sister State.]

THE study of natural history has always formed the favorite occupation of my life. It has been to me a source of unmixed and endless pleasure; and in its pursuits I have rambled over mountains and hills, over plains and marshes, over the cultivated field and the desert. Those unacquainted with the beauty and all-absorbing interest of the subject, have wondered what could be found to repay my toil, or to attract my curiosity; they would often turn aside with a look of contempt at my folly, or seem to pity my infatuation, which they could only attribute to a weakness of intellect, or aberration of mind—others have laughed at my explanations, and thought that I was merely attempting to delude them in my simple answers to their inquiries, when in reality, my search was for gold.

Of the various branches of natural science, mineralogy and geology have had with me peculiar attraction, and hence has arisen that ardent wish which I experience, to become acquainted with the various formations and the mineralogical characteristics of the country which I have chosen for my present, and I hope, permanent abode. Some of these rambles have been taken over different parts of our most interesting State, by which means I have become acquainted with some of its natural advantages and productions.

On my return, my observations have been communicated to many inquisitive and intelligent friends; the opinions which have been expressed, drawn from the highly interesting and important indications of mineral wealth which I have met with in these very limited surveys, have been deemed by them of sufficient consequence to interest the people at large. I have been persuaded, therefore, as the best means of communicating these facts, and spreading them abroad, to bring the subject in the shape of a public discourse before the Legislature, with the view that your honorable body might become acquainted with what is already known, and take such steps for its future development as the vital importance of the subject demands and your wisdom may direct.

It was remarked by a man as eminent in literature as he is distinguished as a statesman, (Mr. Edward Everett,) at a public dinner given to him by the citizens of this town, that "When the next census shall be taken, the valley of the Mississippi will probably be found to contain a population larger than that with which the old thirteen States plunged into the revolutionary war"—and, continued he, "when after another period of ten years, yet another enumeration shall be made, the West will then probably outvote the East in the counsels of the nation. The sceptre will then depart from Judah, never to return.

This prediction was not uttered as an empty compliment or a mere piece of flattery, but it was based on fact and observation, viz. the wonderful increase of population in the western States, which is altogether without precedent, and the posses-

sion of a soil and climate which in fertility, extent and salubrity, has no parallel on the globe. The forests of our boundless region, are daily falling before the ceasless stream of civilized emigration; and populous towns are springing up, like an exhalation from the soil, changing the wigwam of the savage for the mansion of the white man, and the howlings of the wild beasts for the busy hum of social life.

Although this flattering prospect must, under any circumstances, be eventually realized, yet its progress will be considerably accelerated by an early knowledge of the internal resources of the country, and all the advantages that will result from their proper application to the wants and necessities of the people.

If we examine the map of the United States, we see that our State forms the centre of the Union; its central position, and its numerous advantages will sooner or later make it one of the most populous. But many of the advantages which will give it a pre-eminence above the other States are not known. Our inhabitants are generally well acquainted with the nature of the soil, and well know how to appreciate this advantage; but although this be one of paramount benefit, yet there are others of no less importance which are not at all known, and which may be lost or for a long time neglected, if not examined and brought to light by persons equal to the task.

The inhabitants ought to be well informed not only of those parts of the State which are best adapted for the culture of our various products, but they, also, should know those spots which could be more advantageously employed for various other branches of industry, that they may be enabled to secure to themselves the full benefit of those gifts which nature has so bountifully bestowed on the State. Saltpetre has been already drawn in considerable quantities from our caves, and, although manufactured in a very expensive and wasteful manner, has nevertheless been the means of bringing into the country very considerable sums of money. It was communicated to me by an inhabitant of this town, that in the year 1813, two houses in Nashville purchased saltpetre to the amount of upwards of $150,000.

It is all-important that those places should be made known which are particularly fitted for the establishment of such manufactories as require a large amount of materials and which may go hand in hand with the cultivation of the soil; others again are more advantageously established where the raw materials are found, and where the transportation of the manufactured articles to an extended market can be easily effected. To unite these advantages, so that the erection of such establishments may answer the purposes for which they were intended, and being profitable to their proprietors, may become permanent, requires an intimate knowledge of the localities of the country and of the natural products which it contains: this can only be arrived at from an actual survey. Allow me for a moment, to call your attention to the absolute necessity of making the localities of a manufacturing establishment a matter of primary importance to its ultimate success.

I would not recommend aping our transatlantic brethren, but it is necessary that we should become acquainted with their works and improvements; for by a knowledge of the mistakes committed by them we shall be enabled to avoid the expense and perplexity of falling into the like errors.

Since the science of chemistry has been applied to the arts, and introduced into the workshops of the manufacturer, all the branches of human industry have assumed a new appearance, and we may say that chemistry in this point of view has become the luminary which man has suspended in the sanctuary of the arts to clear up all its details.

Before this science had made us acquainted with the general principles, according to which the numerous operations of industry were performed, the manufactories belonged exclusively to certain nations and were the property of few individuals; the most absolute secresy veiled every process in mystery; the formulas and modi preparandi were transmitted as an inheritance from generation to generation. Chemistry has brought to light all these secret processes, and having cleared up the mist by which the arts were shrouded, has made them the inheritance of every one. It will be found that every nation

where this science has been cultivated, has enriched itself by various establishments which were before unknown.

The preparations of lead and mercury were formerly manufactured only in Holland; those of copper, the works of iron, the making of steel, the preparation of acids, the printing of calico, the manufactory of porcelain and many others which were kept in profound secrecy by a few nations, have now become the common property of all mankind.

But though we are acquainted with all these operations and though we may stand in great need of the various articles which are produced by them, it would not be advisable to introduce them indiscriminately into a new country. In order to make an establishment prosper it is not sufficient in choosing its site, that labourers, fuel, provisions, etc. may be easily procured, but it must be situated near a convenient market; it is therefore not upon every spot that such establishments can be erected; the most appropriate places for manufactures have been pointed out by nature herself, each particular branch has its own dominion, and it is not without great inconvenience that this natural order can be inverted.

This influence of position is particularly perceptible in those kinds in which the products are of low price, or in which the manual labour is of small value; these must be established in places where the means of living are abundant and near a convenient market, because they are not able to support freight on the raw materials nor on the fabricated articles. But in light articles of luxury the place of manufacture is of less importance: for instance, a common pottery must be located where there is plenty of good clay and whence the earthen ware may be easily transported to market; whilst a manufacturer of porcelain may prosper in the centre of a large city, because the manual labour in this article forms the great expense; the bulk is comparatively speaking small, and the cost of the raw materials trifling.

Those undertakings which require a large number of hands are perhaps better located in large towns, towards which men are drawn by their mutual wants, and where the majority sub-

sist by industry alone—there it is that those amongst the arts which demand much experience, skill and taste prosper the best, because it is there, that all these various resources may be found combined.

All places are not equally adapted for the location of establishments even as it regards provisions and materials for labour; there are places which although combining all these advantages, are not calculated for certain kinds of manufactures owing to the peculiar nature of the soil. For instance, the land which offers great advantages in an agricultural point of view and produces an easy support, is best calculated for such manufactories as are allied with the natural products of the soil: it is upon these principles that the works of Hemp, Wool, Cotton, Silk, Flax and Wine, far from being noxious to agricultural pursuits, multiply its resources if regulated in such manner as not to employ the hands when most required in the field. Mechanical arts or the manufacture of luxuries would drain the prosperity of such a country to the very root.

It has happened that establishments have prospered for some time in places which seemed unfavorable for them: this prosperity was but forced: the fortune of the owner, the superior intellect of the conductor, the favour of government may have supported their existence for some time, but as it is not in the power of man to change the face of nature, or to annihilate the causes which are constantly in operation, these works which wanted nothing but a proper location will, notwithstanding, after a longer or shorter struggle, fall into decay. Thus the glass houses and iron works which are situated at a great distance from sufficient supplies of ore and wood and coal, can only be short-lived, and from their birth, point out the folly and ignorance of their projectors.

We have remarked above that establishments for the preparation of luxuries and fabrics of fine stuffs, could only prosper in large towns where the collection of a large number of individuals, offers many resources which would be unattainable elsewhere. But how dearly are those advantages bought by the inconveniences which every where attend the collection in

towns of large numbers of the working class. Let us look at the present state of Europe, where we may see the terrible consequences of such a state of society. Twenty or thirty thousand families may have their existence essentially depending on the prosperity of the factories! A political revolution, a change of taste or of fashion, a deficiency of the necessary supplies, a declaration of war may paralyze their activity, and almost in a moment put an end to their employment, thereby jeopardizing the lives of forty thousand individuals, and dooming them to anguish, misery, despair and famine.

I have always considered these alarming accumulations, as the greatest scourge in the progress of civilization, and I believe it is a wise and prudent policy to prevent them; they not only every moment threaten the public tranquility, but they endanger the stability of the works themselves, because they expose them to changes which act so powerfully upon the population of cities.

If we again turn our attention to Europe, and take in view the manufactories which have had a continued prosperity, and whose existence has not been influenced by the tempests of revolutions, nor the caprices of fashion, nor the versatilities of laws and commercial regulations, we shall find them all in countries where the aridity of the soil did not require the assiduous labour of the inhabitant; and experience has shown that though in the humble cabin of barren mountain districts, the means of execution are less perfect than in towns, nevertheless, that the articles there manufactured are brought to all the markets of Europe at a lower price than those prepared in towns: this is owing to the price of labour being considerably less in the former than in the latter places.

From what has been stated, you will observe that it is of the greatest importance that the inhabitants of our new States, should be acquainted with the natural capabilities of the different parts of their country, that those fit for agriculture should not be selected for the purpose of large manufactories, but on the contrary those mountainous trates should be chosen which are often furnished with fine mill seats, with immense forests,

and where are often found large quantities of raw materials. The State of Tennessee is rich in such localities—her mountainous districts abound no doubt in various articles which will one day form the wealth of our state; but these facts can only be ascertained and made known by a proper scientific investigation. I have only as yet visited a small part of the State, but from this trifling survey, I am convinced of the truth of my assertions. I look forward with full confidence to the time when our Cumberland mountains, rich in excellent iron ore and coal, will become the Birmingham of the West—their streams furnish numerous fine mill seats, and their caves abound in materials for the preparation of magnesia, Epsom salt, and saltpetre.* The part of East Tennessee which I have rambled over is rich in mineral productions. I found an excellent quality of that zinc ore of which in Europe the best brass is manufactured. In several places lead is found, and there is every indication of marble of as many varieties, and equal if not superior to the finest Italian. I have seen places where

*The United States probably does not afford a more eligible situation for the location of an armory, and the erection of the necessary foundries for the manufacture of cannon and small arms, than is to be found in that part of the Cumberland Mountains situated between the eastern Tollgate and the Tennessee river. This position may certainly be assumed when the improvements now going on for rendering the river navigable shall be completed. In the situation above alluded to, there is abundance of fine coal, and an inexhaustible quantity of good iron ore, not more than two or three miles from the Tennessee; and the position of the country is such as to form a gentle descent from the mines to the river, so that a rail-road could be placed there at a trifling expense. In the vicinity there are found several fine mill-seats, and considerable spots of highly fertile land, sufficient to support any number of hands that may be required.

It possesses the blessings of healthiness in an eminent degree, and a very dry atmosphere, so highly important to preserve the armour from rust. In such a situation, no interruption in times of danger and need would take place; the production of naval and military munitions of war might be carried on to an almost unlimited extent, because it is almost impossible for an enemy to penetrate into the very centre of the Union.

In the years 1813 and 1814, in the neighbourhood in which I lived, was the foundry of Mr. Dorsey, situated near the mouth of the Patapsco, where the cannon for the Navy were cast; but after the commencement of the war, when the guns were most required, in consequence of this establishment being within the reach of the enemies' shot from their vessels then occupying the Chesapeake bay and Patapsco river, all its operations were necessarily suspended. And it is my opinion, from the local advantages possessed by the Cumberland Mountains, that the castings could be there made at 50 per cent less than at the Patapsco furnace.

excellent quarries of roofing slate may be opened.* I found several localities of manganese, and rich magnetic iron ore. Also much appearance of salt along the foot of the Cumberland mountains, and I am informed, salt has been discovered in the Western District.—I will not mention the gold found in several of the counties of our eastern extremity—because I consider our iron and coal as constituting the solid and permanent mineral wealth of the State—in support of this opinion I might mention England and various other countries on the Continent of Europe.

Coal is one of those gifts of Providence which we cannot too highly appreciate. It may be employed for all domestic purposes without fearing those noxious qualities which some persons have attributed to it. The sulphureous vapours exhaled by some varieties during combustion are not to be dreaded; they are owing to some accidental intermixture of pyrites, from which, when necessary, it may be easily separated; pure coal does not contain an atom of sulphur. There are other considerations which make it of importance that proper care should be taken of these mines, even in the United States, and

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* It is a matter of much surprise, that the use of roofing slate for the covering of houses has not been more introduced in our large cities. Notwithstanding we hear the cry of fire so often in Philadelphia, New York, and other places, the houses are still covered with shingles, which are soon destroyed by the weather, and when dry, so easily take fire; whereas a slate roof, when made of good materials, will last a hundred years, and is incombustible.

The mountain range which separates our State from North Carolina, includes several localities where slate exists, particularly near the head of Little Pigeon river, in Sevier county. The slate of that vicinity, judging from the hasty examination to which I could submit it, (from the shortness of my visit) seemed to be of excellent quality for roofing, which is not the case with every kind of slate.

The sort best calculated for roofs is susceptible of being divided into thin and straight lamina. This quality is lost when the slate has been quarried for some time, and therefore, those parts of the quarries which are exposed to the action of the air, cannot be used for this purpose. Slate for roofing must be compact, and such as will not absorb water. This may be ascertained by plunging a piece of the slate into a vessel of water, so that a portion of it may be above the water; if after having remained in this position about twenty-four hours, there be no appearance of absorption or moisture reaching more than one third of an inch above the surface of the water—or should its weight be but little increased by complete immersion for some time, the slate is considered good.

The soft slates are soon destroyed by humidity and frost—if it contains pyrites, it is not fit for roofing, because the decomposition of that mineral substance destroys it.

in our back woods, namely, for the future use of our iron works and steamboats which consume such great quantities of wood. So long as our state shall remain thinly settled, we may have plenty of forest land to supply all these wants; but in proportion to the increase of population, the forest will diminish and the use of combustible materials increase. England would not be the rich country she is without her important coal mines. France has for some time been imitating England in her use of coal; about thirty or thirty-five years ago the famous founderies of Creusot near Mount Cenis and Autun have been established by the use of these materials. And to coal Pittsburgh is indebted for her manufacturing celebrity.

When coal has undergone a certain operation by which it is deprived of its bitumen, and other volatile matter, it is known by the name of *coak;* it then becomes a species of charcoal and burns without smoke, without caking, without odour, and forms not only the necessary fuel for iron works, but may be used for culinary or domestic purposes where smoke or odour would be inconvenient.

Thus you will observe we have other sources of national wealth of far greater importance than our gold, and strange as it may appear I consider iron as preferable to the glittering and much sought for metal. Iron was considered by the early chemists as the most ignoble and vile of all the metallic substances; whereas, now it is the most important and the most useful of them. Without iron no art could exist; man would have remained in his savage state; he would still have to dispute his food with the wild beast. We should have no agriculture nor would the earth have been broken up by the plough. Without iron no other metal could have been made available to the use of man, because, it is by iron that they receive their various forms and aptitudes. It possesses in itself all the qualities of the various metals, and no other can be made a substitute for it. If scarcity, brilliancy, and indestructibility place gold and silver above iron, the great services which the latter renders to society make it much more highly estimable in the eye of him who does not value things by their external appearance. Na-

ture has not endowed it with a handsome and pleasing color, nor does it glitter with much brilliancy, but she has bestowed on iron more precious qualities. Indeed, the condition of man would be miserable without the possession of this metal; witness the history of those nations who were unacquainted with the art of working iron, and who gave with pleasure the gold with which their soil abounded for iron which more happy nations brought them in exchange. In fact, iron seems to be the standard—the thermometer of the happiness and prosperity of nations. If the poets have characterised the first epoch of corruption and misery among men by the iron age, the philosopher and the philanthropist on the contrary, consider the time when this metal was first wrought as the true revival of the human species. In their estimation the numberless benefits which the working of iron has produced infinitely overbalance the difficulties and labor which are necessary to procure it.

We must therefore consider iron, particularly in the vicinity of coal mines, as a permanent source of prosperity to a country, and such treasures in *juxta position*, are possessed by Tennessee in abundance. It is therefore of the highest importance that a perfect knowledge should be obtained of the various situations of the coal and iron mines, both for public and private advantage, and particularly those of coal, and that they should be opened and worked in a proper manner, or they will be ruined; this I found to be the case with many of the lead mines of Missouri. The proprietors of the valuable mines of coal ought not to have it in their power to destroy them, which will certainly be the case if improperly worked. Persons ignorant of mining take the most easily accessible part, and encumber the remainder with the rubbish of the adjoining rock and the refuse coal, rendering the mine after the easily attainable coal has been removed, hazardous to work and much more expensive to penetrate in its lower parts, and thus their use is lost to future generations. It is therefore of the highest importance that this kind of property be placed under proper regulations which would put it out of the power of the present owner to

deprive his successors of the benefits which under proper management would accrue to them.

In order that all such resources may be known and the proper regulations made to derive all the advantages possible from them, it is necessary that the natural productions of the state should be investigated, that a mineralogical and geological survey of the country should be performed, which when properly executed must prove of great utility to the inhabitants, by giving them an opportunity of becoming acquainted with the nature of their lands, and enabling them to turn to profitable use many articles which now are rejected and considered useless. Various branches of industry could be introduced in spots at present abandoned and considered unproductive; this is particularly the case with the slate and marble quarries, capable of becoming very valuable branches of industry in some of those districts where the fertility of the soil would not be sufficient to support the settlers.

Perhaps the utility of a geological description of the country is not fully understood. I do not pretend to say that a mere scientific description of the different formations, of the various alternations of the strata, of their probable origin and relative age, etc. should be drawn up. All such considerations are highly interesting for the speculative sage, but will not profit the inhabitant of a new country. It is known, for instance, that what is generally understood by formation is meant a collection of rocks which are formed at different periods, and of which each contains its proper metals and other useful materials being distinguished by some characteristic property. For instance, in the primitive formation we find tin, molibdena, tungsten, and some others; they are only found in such formations, and it would of course be a folly to look for them in more recent ones.

During my investigations, I have discovered in our State, several groups of rocks belonging to the transition and secondary formations. There is no rock formation which contains so many valuable metallic deposits as the transition. We have, for instance, in the slaty rocks of that formation, the

lead mines of Lead Hills, and of Wanlockhead, in England; the celebrated mines of the Hartz, which produce yearly, sixty thousand quintals, or six millions pounds of lead, and a considerable quantity of silver; a large number of the mines of Hessia, of Nassau, of the Ardennes, &c. are situated in this rock—the gold mines of Vorespotack, in Transylvania—the mines of silver and lead of Bretany, particularly those of Poullaoen and Huelgoat, in France, are situated in the same formation. In Hungary, the transition formation is so rich in various metals, that the rock has been called *saxum metalliferum*, (metalliferous rock).

Several of the rich veins of Mexico, as those of La Veta Madre, at Guanaxuato, also those of Potosi and of Zacatecas are in similar rocks.

The transition limestone produces the finest marbles. The *rosso corallino* of the Italians, is the transition limestone of Blankenberg; the *nero d'egetto* of the Italians belongs to the same formation—also the black marble of Nameur and Dinant—also the marble granite or petit granite—in fact, nearly all the celebrated variegated or ornamental marbles used in architecture, belong to the transition class, while the statuary marbles are of primitive and the coarser marbles of secondary formation.

Of the secondary formation, the coal is the most interesting member; and besides iron, which is abundant in nearly every coal formation, we find also lead and sulphuret of zinc, as in the coal mines of Northumberland and Wales—the mines of lead at Potshappel, near Dresden, and copper ore in the coal mines of Silesia.

The oldest sand stone of the secondary formation, which is abundant in this State, is particularly rich in iron. In the Uralian mountains, in Saxony and Lorraine, it contains rich copper ore. Many of the lead mines of England and of those of Vosges, are in the old sandstone. But the most interesting that I am acquainted with, are the mines of Bleirberg, near Aix-la-Chappelle. The quicksilver mines of the Palatinate and Deux-ponts are in the same rock.

Such are the products which in Europe these formations contain; and it is more than probable, therefore, that a proper investigation may bring to light similar substances in the similar formations of our country. Humboldt in his journey through Brazil examined the gold and diamond regions of that country; on his return to Europe in travelling over the Uralian mountains, he found there exactly the same formations which he had before seen in Brazil, and from that circumstance alone was led to predict that sooner or later gold and diamonds would be found there also; his prediction has been already verified, both gold and diamonds being found in the very situations which he had indicated. These facts are surely sufficient to point out the propriety, if not the necessity, of a geological survey of this promising State. But there are other motives worthy of consideration. It must be obvious to every superficial observer, that the fertility of the soil varies very much in the different parts of our State—that in some places cotton, for instance, can be brought to a high degree of perfection, where the soil is not adapted to the cultivation of wheat. In other places wheat and rye may be produced where it is not fit for cotton, grass, etc. If we examine the nature of such soils, we shall find them to differ in their constituent parts, owing to the variety in the compositon of the rocks from which they are formed—of the nature of which they always partake. This may be observed by every one who travels from the western towards the eastern extremities of our State. He will find on this side of the Cumberland mountains wherever the horizontal stratified limestone prevails, a fertile, stiff soil—where the hornstone and sandstone prevail he finds it light and not as fertile. On the other side of the Cumberland mountains, he finds a soil different from either of the above. There it is more impregnated with iron, and though rich, not so well adapted for the culture of those articles which grow luxuriantly on this side. He will also observe that the rocks are of a different nature. Although some do not differ in their chemical constituents, their position is not the same; the strata do not lay horizontal, as is the case on this side, but they approach very much to the

vertical, and he will find the soil is of a different nature, as well as the plants and trees which grow upon it.

This influence of the rocks upon the nature of the soil and its products, is very perceptible in our vicinity. The generality of our rock is limestone. This limestone is not pure carbonate of lime, but contains upon an average from several analyses which I have made, about 25 per cent of other materials, principally alumine with a small quantity of silice and iron, being the same constituents as the generality of our clays: now this kind of limestone is very susceptible of decomposition; the carbonate of lime is dissolved by the rain, and the aluminous part being insoluble forms our soil, the impalpable dust which covers our roads during dry summers, and the horse-killing mud which is found every where during the rainy seasons. A journey from Nashville to the Harpeth river will convince any one of the truth of my statement. The first 6 or 8 miles is on the limestone rocks with a heavy soil; then follows a stratum of shale, too small to have any influence upon the formation or nature of the soil; but this is covered with a stratum of sandstone and now is perceived a great change in the soil, it being light, very sandy, and not at all fit for the same agricultural purposes, which the soil of the limestone country is adapted to. This decomposable quality of the limestone gives rise to the numerous caves which we find in the western country—the water passing at first through small crevices enlarges them by dissolving their sides and carrying off the insoluble parts to the rivers; so that in the lapse of ages, this seemingly trifling and insignificant operation, has produced those astonishing and extensive chambers and caverns with all the tortuous and winding passages which are sometimes found to continue for miles through a solid bed of rock. It is also owing to this soluble quality of the rock that our springs are so much impregnated with lime.

Perhaps it will be asked, why the same circumstances are not found to obtain on the eastern side of the Cumberland mountain range where limestone abounds? The answer is, that the limestone of that part of Tennessee has not the same

constituents as the rock of our vicinity, but is pure carbonate of lime, or very nearly so, and being of a more homogeneous composition is not so soluble. Much of the limestone of this vicinity is so greatly contaminated with heterogeneous materials that it is unfit for the best plastering works; and our mechanics are obliged, for their white coat, to get lime from some particular spots. The limestone from East Tennessee, if susceptible of being burned, becomes almost entirely white lime.

In addition to these paramount considerations which the geological investigations of a country may bring to light, this science possesses many other advantages.

Geology is the luminary which guides the miner in his subterranean routes—indicates to him the degree of probability with which he can proceed in his search after such metallic or such combustible mineral in each particular formation—and points out the way which he must follow to find again the vein which he was working and had lost.

A knowledge of the great mineral masses is necessary to the civil engineer in surveying his routes for canals, rail roads, or turnpikes. He must know the nature of the country through which he is to establish his works, the materials to be obtained, the circumstances of its stratification, the greater or smaller resistance it will oppose to the deleterious action of the atmosphere, or the pressure which is to act upon it.

The geographer whose business it is to make known the physical position of countries and the configuration of their surfaces may also derive great advantages from the science of geology. The mineralogical constitution of the soil exercises a great influence upon the size and the form of the mountains and hills, and in consequence of this, upon the quantity and force of waters in the rivers, and as we have already stated it has the same relation to the vegetable soil, and consequently to the productions and population of a country.

Permit me now to enumerate some charms in its study which have contributed in no slight degree to its progress. This science not only engages the mind but it requires also such exertions of the body as is pleasing and healthful to the

young observer. The obstacles which he often has to conquer, the dangers he has sometimes to dare in attaining the object of his excursions, excite his courage and provoke his ambition to acts of noble daring—the exercise gives strength to his body and elasticity to his muscles; thus he gains in the same pursuit robustness of body and cultivation of mind.

Saussure, conceiving himself in some measure challenged by Mount Blanc presenting to him always a summit he had often endeavored in vain to assault, redoubled his exertion; and it is perhaps owing to this ardent desire to triumph in his enterprise that we are indebted for the beautiful observations which this naturalist has made upon the highest mountain of Europe. After having conquered a legion of difficulties and troubles, the observer of nature at last arrived at that astonishing height which commands the delightful prospect of a whole country. He feasted his eyes upon the most charming picture! The whole universe seemed to be at his feet. Man and his monuments appeared in all their littleness and insignificancy. His mind, participating in his elevated situation, felt itself exalted above all those little passions—those petty interests which move mankind; there he experienced the most delightful sensations of which the mind of man is susceptible. With regret he tears himself from the scenes which have produced so much delight, but always remembering them with pleasure, the desire was continually renewed to ascend other elevations and to augment thereby the riches of the science to which he was devoted.

Animated by a similar desire to contribute my mite to the advancement of the same science and the prosperity of this country, I have ventured thus to intrude, gentlemen, on your important occupation.

ART. III.—*History of a Case of Sarcomatous Tumour of the Mamma—its extirpation, &c.* BY N. HITT, M. D.—*of Vincennes, Indiana.*

VINCENNES, IND. Nov. 10, 1831.

MESSRS. EDITORS,

AT the semi-annual meeting of the Medical Board of the first medical district of Indiana, held in Vincennes, November 7, 1831, the subjoined history was presented by Washington W. Hitt, M. D. of Vincennes; and the following resolution adopted.

Resolved, That the history of the case just read by Dr. Hitt is highly interesting to the surgical community, and worthy a place in some of the medical periodicals of the day; and that it shall be the duty of the Secretary to transmit to the Editors of the Transylvania Journal of Medicine, Lexington, Kentucky, and to the Editors of the Western Journal, Cincinnati, Ohio, each, a copy for publication.

By order of the Board,

H. DECKER, *Secretary.*

The subject of this history is a Mrs. Holmes of this place, aged twenty-four years. The previous history she gave me of her affliction is as follows.

About twelve years ago, when her system commenced its expansion, she discovered her left breast gradually to increase in size, and to continue to grow after the system had received its ultimate development, giving her no pain and consequently no uneasiness or alarm, until after she was united in marriage to her present husband. During pregnancy, she thought it increased more rapidly; and after she was delivered of her first born, its increase became more evident and alarming. A few days after the birth of the child, the gland had every appearance, as she thought, of having secreted milk, and was so much distended that medical advice was thought necessary and was obtained. The child was denied access to the enlarged breast, and stimulating applications prescribed with the view to suspend the secreting process.

About this time she thinks a redness appeared circumscribed

to about the size of a half crown, one inch to the right of the nipple; which redness or inflammation soon gave place to suppuration, and discharge of a fluid, the colour and consistence of which she does not now recollect. After discharging a few days, it entirely healed, and no perceptible change was discovered until the second accouchement. Between her first and second pregnancy, her medical attendant, she states, was as assiduous as unsuccessful in his endeavours to discuss the tumour, by the exhibition of medicine internally and externally, until ptyalism was produced and continued for some time.

On Christmas last, she was delivered of her second and last child, when the tumour again resumed its progressive march, and by its accelerated action and unyielding disposition, produced in the mind of the patient the most awful apprehensions.

The following April I first saw her, having been called to relieve her of an attack of fever, at which time the tumour was shown to me as a curiosity. And, what will be thought surprising in a subsequent part of this history, the child was then applied to the enormous breast, which, from its magnitude, looked very unlike the mamma. I then had occular demonstration of the fact, that secretion of milk was performed, and the child's healthful aspect itself proved it to be amply supplied with the pabulum of life. The tumour at this time was not particularly examined, as it was not requested.

I did not see the patient again until the first of July following. She was then much alarmed in consequence of a circumscribed redness, the size of a crown, appearing about four inches below the nipple, and which having subsided, left to the feel of the finger evident fluctuation. The external appearance of the tumour was smooth, uniform, and elastic, possessing but little sensibility and slight soreness on pressure. Upon pressing the sides of the tumour with the fingers, the internal structure imparted an uneven and irregular feeling. Its largest circumference measured thirty-two inches, and its base twenty-two. Its supposed weight from fifteen to twenty pounds. It was supported by a bandage from the neck, assisted by the left arm and hand, which were constantly applied beneath it. None of the neighbouring glands were implicated.

Considering the circumstances of the case, the age and comparatively good constitution of the patient, I gave an opinion that extirpation was not only advisable, but would be the only possible means of her salvation from an untimely grave. She did not fully determine to submit to an operation until the above opinion was corroborated by the opinion of several gentlemen now present, who did me the kindness to examine the tumour.

Her mind being fully made up, the 20th July was appointed for the operation. From an unyielding desire to undergo the painful ordeal among her nearest kindred, from which she could not be dissuaded, she was conveyed in a carriage twenty-two miles north of this place a few days previous.

Every preparation being made, my friend Dr. Somes and myself, set off on the 19th, and on our way met an acquaintance just from the patient, who stated that nature had anticipated us and had performed the operation by the tumour's bursting and discharging. We went on, however, and found the tumour collapsed, and still discharging gradually a fluid, the colour and consistence of which resembled thick rich cream; and, judging from the vessels containing the fluid, and also the condition of her cloths, there must have been at least one gallon discharged.

The fluid had a pulpy feel, and peculiar smell, not offensive, however. The probe could be plied in several directions, and in some it was not of sufficient length to determine its boundaries. The minds of the patient and her friends were much relieved, supposing the late occurrence would supercede the necessity of an operation; and in vain did we endeavor to convince them otherwise. We deemed it useless to insist on an operation under existing circumstances, but contented ourselves with controverting the notion that its discharge was a sufficient reason to delay the operation, and told them it was probable she would be placed under less favorable circumstances for an operation when it would be absolutely necessary for one to be performed.

After expressing our views, we passed on to Terre Haute.

While there our patient was taken with a remittent fever, and during the second exacerbation of fever the blood-vessels of the tumour gave way, and an alarming hæmorrhage ensued. A messenger was immediately dispatched, and we were hurried back to the patient. July 26th, we found her much exhausted from fever and loss of blood, and still bleeding.

In consultation with Drs. Somes, Davis, Ohowver, and Elliott, it was agreed that an operation was the only possible expedient to save her from a speedy dissolution; and that even that was extremely doubtful under existing circumstances; such as her great debility, considerable fever, the quantity of blood she had already lost, her mind agitated with fearful apprehensions, and the tumour collapsed into a loose mass, all tending to an unfavorable prognosis. By the assistance of the above named gentlemen the operation was commenced in the usual way of extirpating cancerous breasts, by making two semicircular incisions, dissecting back a sufficiency of the integuments to cover the exposed surface after the removal of the tumour. The tumour was then cautiously dissected from above downward; great care being taken to tie the arteries as they were exposed. Thus the whole tumour was carefully and entirely removed leaving the pectoral muscle bare. The patient was then laid on the bed to recover from syncope, the flaps lying open and the whole covered with lint.

I would here state that our patient was remarkably courageous and bore the operation with a fortitude seldom witnessed in the most extensive operating theatres. She was supported by wine and water as occasion required. After she had somewhat revived, and finding no hæmorrhage on raising the lint, the flaps were properly adjusted and retained by adhesive straps. The compress and bandage completed the dressings. She was then cheered, encouraged and congratulated, which elicited from her a smile prospective of returning health, and all faces beamed with gratitude and thankfulness for such a deliverance.

In a few days her fever subsided, the healing process commenced and kindly progressed. Three fourths of the flaps

healed by the first intention; and in three weeks from the day of operating, she returned to her domestic circle; when three weeks more completed the entire healing of the wound.

Viewing her situation previous to the operation, when she was prostrate beneath the arm of disease, and to all appearance on the verge of dissolution, and seeing her now, reinstated to health, family and friends, we cannot but consider this case a striking illustration of the powerful influence of our art in meliorating the condition of man, and a conclusive confutation of the vulgar prejudices entertained against it by some.

DISSECTION OF THE TUMOUR.

The tumour was opened by cutting through a thick capsule formed apparently of condensed cellular substance. On its surface next the skin the capsule firmly adhered. The substance of which the principal part of the tumour was composed, was made up of irregularly shaped masses, in colour and texture somewhat resembling the masses which compose the pancreas, and appeared to be connected with each other by a fibrous substance of a looser texture.

A very small portion extending from the nipple to the clavical margin of the tumour, bore the appearance of healthy gland. This was half an inch thick at the nipple, and gradually increased in thickness as it ascended to the margin, and composed perhaps one twelfth part of the whole separate from the fluid. It is remarkable in this case, considering how small a portion of the whole mamma was in a healthy state, and shaped as it was, that milk was pressed from the nipple only two weeks previous to the operation.

The cavity of the tumour occupied its centre, extending principally below and three or four inches above the centre.

ART. IV. *A case of Comminuted Fracture of the Skull, with loss of Brain and Fungus Cerebri.* By ABRAHAM ADDAMS, M. D. *of Cynthiana, Kentucky.*

ON the 6th of September I was requested to visit a boy, eight miles distant, who had received a kick on the head from a horse. The messenger had not seen the boy, and knew nothing about the extent of the injury; but had been informed that the skull was not broken; and therefore I did not take trepanning instruments with me.

On my arrival I found the boy, who is about nine years of age, very restless, writhing and twisting continually like a wounded worm. He could not be held still—his eyes were permanently open, his pupils dilated, his pulse irregular, his respiration somewhat laboured. On removing the coagulum of blood which filled the depression in his skull, I ascertained that there was a comminuted fracture, with a rupture of the membranes, some of the brain having been discharged through the rupture, and every violent exertion, causing a continuation of the discharge. I immediately despatched a messenger for trepanning instruments. In the mean time, I placed my hand over the orifice in the membranes, to prevent, when the patient made much exertion, any farther loss of brain.

The boy was not seen at the instant the blow was inflicted. The stroke was heard by some persons who were near; looking in the direction, they saw the boy falling, and the animal with the appearance of having just kicked. From the seat of the injury the boy must have been stooping forward; he had also an extensive contusion on his hip and on the left arm, near the elbow. The presumption is that in leaning forward, (which would bring the elbow to the hip, and the top of the head in a situation to receive the blow) the animal kicked with both feet; that one struck the head, and the other the contused arm and hip. The mare no doubt kicked with her utmost force, as she was at the time teased by a stallion.

In about two and a half hours, having procured instruments, I proceeded to the operation. I made a crucial incision in the scalp, turned up the flaps, and removed all the detached portions of bone. The fracture was seated in the left parietal bone, extending in length about two and a half inches, near the middle of the bone and nearly at right angles with the sagittal suture. A line drawn from one ear to the other would pass over it a little obliquely.

The removed portions of bone, when placed together, formed an irregularly shaped figure. In the centre it was about one and three-fourths inches in width; at the extremity next the sagittal suture, not more than three-fourths of an inch, and of an angular shape; at the opposite extremity, it was one and a fourth of an inch in width, of an irregular circular form. At this extremity the plates of the bone were separated, and five spiculæ of the under plate driven through the membranes into the substance of the brain. I saw one partly buried in the brain, which led me to seek for the four that were entirely concealed from view. I introduced my finger, felt the spicula, and making my finger a director, followed it with the forceps, seized the bone, and extracted it. One of the four removed in this way was three-fourths of an inch in length, rather more than half of an inch in width, having irregularly serrated edges. The rest were of smaller dimensions. From the extremity last mentioned a fissure extended into the occipital bone, the extent of which appeared considerable. I did not trace it far, as no necessity existed for it.

About half an inch from this extremity, there was a depressed portion of bone of a semicircular form, not more than half an inch in its widest diameter, separated from the main fracture by a space of sound bone, not more than half an inch in width, connected with and situated entirely on that side of the fissure, before spoken of, next the occipital. From the situation of this depressed portion, it was impossible to apply the elevator so as to raise it, and as the scalp was lacerated, over the depression, I determined to remove it with the trephine. How this small portion was depressed, separated from the other

I did not entirely satisfy myself: the most probable conjecture to me was that it had been done by the heel of the horse's foot, or some irregularity in the hoof, the animal being unshod.

The other fractured portions were so completely detached, that there was but little difficulty in removing them, except a portion near the middle of the fracture, in its widest part. This portion was divided equally in a longitudinal direction. The middle edges were driven in, and very tightly wedged together; and the outer edges being irregular, and the under plate wider than the upper, rendered its removal very difficult. I however succeeded, by clipping off the irregular projections in the upper plate, at several points, and then elevating the centre. From appearances, corroborated by the statements of those who had been with the boy from the beginning, it was evident a considerable quantity of blood had been lost. In making my incisions I divided a branch of the occipital artery, which I regarded as a fortunate circumstance, and permitted it to bleed, until from the quantity lost, I thought it prudent to check it by pressure. During the operation there were several ounces of blood lost from branches of the middle artery of the dura-mater, that were ruptured, their impression on the removed bone being very distinct.

After having removed all the detached bone, and the coagula that formed in the depression, I brought together the edges of the divided scalp by a few sutures, leaving the part entirely open from which the bone had been removed. During the operation he gave no symptoms of suffering except when I made the incision in the scalp; the force of several men was then necessary to hold him.

I placed the patient in bed, having his head elevated; he lay quiet during my stay that evening, which was not more than an hour after I had him removed from the table to the bed. The restlessness and contortions of his body which had been noticed, no longer continued. He seemed disposed to sleep. His pulse and respiration were not much improved. The boy slept some during the fore-part of the night; but in the latter part fever having supervened, he was bled to eight ounces.

On the morning of the 7th I saw him at 10 o'clock, and finding his skin hot, and his pulse active, I took nine or ten ounces of blood; gave him pulvis jalap.; directed it to be repeated every three hours, until three portions had been given. If that did not operate, then to give oleum ricini, and if in two hours, no operation was effected, to administer an injection. The boy had not yet spoken. Yet he seemed to understand the queries put to him, though he did not speak. My directions were strictly followed, without producing the desired effect. The patient became so extremely restless, and unmanageable in the latter part of the night, that a messenger was despatched for me. On the morning of the 8th, I arrived at an early hour, and found my little patient very untractable. Unless held, he would roll over his bed, and make efforts to get up. The injections had been retained. I immediately attempted the introduction of a suppository of soap, which on account of the spasmodic contraction of the sphincter was not easily effected. In a few moments the soap was discharged, followed by a copious evacuation of fæcal matter.

As no urine was discharged, and his restlessness continued, I was led to examine his bladder, which was found to be greatly distended. While reflecting on the best mode to effect a discharge (not having a catheter with me,) the boy made an effort to rise, saying "let me get up." We then raised him to the floor. A profuse discharge of urine followed. He now became quite composed; and answered rationally questions put to him.

A continuation of the cathartic operation was directed. The suppository of soap was found necessary to effect a discharge, for the five succeeding days. After that time it was no longer used, but a cathartic was daily administered, often repeated several times in the day, until the end of the fourth week. From this period to the sixth week, a cathartic every other day was sufficient; after which no cathartic medicine was administered. During the whole course of the treatment, calomel was administered about once a week, in doses of about ten grs.; the other cathartics used, were pulvis jalapæ, pulvis rhei, sulphas

magnesiæ, and oleum ricini; these were varied, repeated, or alternated, according to circumstances; restricting him at the same time to the lowest diet.

In the first instance no other application was made to the wound, than a dressing of lint, confined by proper bandaging. The dressing was soon discovered to be stained with bloody serum, and before the morning of the 7th, a large quantity had been discharged. It became necessary frequently to change the pillow, and the piece of cotton cloth that was used to absorb the discharge. This discharge continued for five or six days, though after the third it was much diminished in quantity. On the fourth day the dressing and lint were removed, when the wound made by the incisions in the scalp, was found to be suppurating, and serum a little tinged with blood, discharged from the part injured by the blow. I renewed the dressings daily for thirty days: after the third dressing I removed the sutures that had not been discharged by the suppurating process, and applied strips of adhesive plaister, that were daily renewed.

On the morning of the 11th the patient became more uneasy and restless than he had been for several days. In a short time he had violent convulsions, which continued for some hours. On inquiry I very satisfactorily ascertained the cause of these convulsions. I had directed them to make a cap of light materials, very large, (to prevent pressure on the wound) to keep the boy from disturbing the other dressings, for he had from the beginning a constant disposition to tear them from his head with his right hand, (his left being paralytic from the receipt of the injury.) This cap, in opposition to my instructions, was made of coarse domestic linen, and so small as to press tightly on the wound. It was put on in the morning and in a very short time, the boy became restless, and could not be kept quiet; the convulsions followed, and were arrested by the removal of the pressure, produced by the cap and bandages. The struggles of the boy having deranged them so much that they were entirely removed by one of the attendants.

The case went on well from this period, nothing remarkable occurring until, as I had anticipated, on the 11th day fungus cerebri made its appearance. I at once determined to adopt the treatment I had heard recommended by Professor Dudley. I enveloped a piece of dry sponge in a strip of gauze, and by means of the bandaging confined it to the part. Having early made this application, the fungus was prevented from becoming large, and in a few days it entirely disappeared.

In the seventh week from the time the injury was received, I visited the boy for the last time. The wound was then cicatrized, except a very small portion, a mere point.

Nov. 14th. The boy in company with his mother came to town to-day, on horseback. The wound is completely healed. He has perfect use of the arm that had been paralysed. His mental faculties seemed not in any degree impaired. His health is perfect. He has been several times to mill, riding on a bag, a distance of five or six miles.

Art. V. *Case of Hereditary Hæmorrhagic Tendency, with Remarks.* By Dr. James N. Hughes, *of Simpsonville, Ky.*

Mr. W. ——, aged 37 years, of ordinary stature, and biliophlegmatic temperament, with a rather feeble and frequent pulse, has been for the last four years, in a crippled condition, the result of a chronic rheumatism affecting the loins, hips, and the articulations of the upper and lower extremities. The parts chiefly implicated at present, are the left hip and knee; the latter admitting of but little motion owing to a rigidity of the tendons, ligaments &c. surrounding it. The parents of the gentleman state, that from infancy he has been much incommoded at times with pain, swelling, and stiffness about the joints; particularly those of the toes and fingers. Since the age of puberty, however, the larger articulations appear to have suffered principally.

So far as I have been able to ascertain, there seems to be no evidence of the local affection in this case having been connected essentially, in its origin or progress, with chylopoietic disorder. The gentleman (to use his own language) has uniformly been "sound at stomach, and healthy in his bowels."

In addition to the rheumatic affection, the case of Mr. W. is strikingly characterized by a very remarkable constitutional predisposition to hæmorrhage; not from a single organ merely, but from all. He at different times, having suffered from attacks of hæmatemesis; hæmoptysis; hæmaturia; epistaxis; and hæmorrhage from the gums, and anus. Such indeed, is the extreme atony of the vascular tunics, that considerable extravasations of blood have, at one time or other, occurred in almost every part of the body, without any appreciable cause whatever. An extensive ecchymosis now exists among the muscles and integuments of the left thigh, for which no cause can be assigned. Manual labour, riding, coughing, &c. have often been followed by ecchymoses; or what the gentleman has been accustomed to call "*inward bleeding.*" For fear of this occurrence, he rarely ventures to sit for any length of time upon a hard chair, without a pillow or cushion to protect him. The slightest cut, in any part of the body, has never failed to produce copious hæmorrhage. A small puncture, made accidentally when a lad, in the lower, and fore part of the thigh, with the point of a pen-knife, gave rise to a hæmorrhage, so considerable and protracted, as almost to have proved fatal. At another time, the extraction of a tooth that had become so loose in its socket, as to be removed without the use of instruments, greatly endangered life, by the bleeding which followed it. Mr. W's. parents are of opinion, that from the age of three to twenty-seven years he must have bled at the nose upon an average, at least once a day. Sometimes the bleeding would continue with but little intermission, for five or six days together, to the great annoyance of the patient, and his friends. But for the last ten years, he has not been so much afflicted with epistaxis, as formerly. Nor has the amount of blood lost, upon the whole, since the

age of thirty, been as great as it was anterior to that period; though the liability to hæmorrhage still continues, and probably to as great a degree as ever. The fact of there being a diminution in the quantity of blood voided and extravasated within a few years past, is easily accounted for by the circumstance of the patient's leading a much less active life of late, in consequence of decrepitude from rheumatism.

In further proof of the prevalence of a general hæmorrhagic diathesis in the case of Mr. W. ——, I will add another circumstance, related to me by himself. About fifteen years ago, he felt for the first time, a dull pain, attended with slight tumefaction, about the middle of the left arm, in the direction of the humeral artery, which was caused, as he supposed, by reaping with a sickle. Finding in a day or two, that no amendment took place, he had poultices applied to the part. In a short time, a globular tumour was observed about the size of a common tea cup, at the point where pain was first experienced, somewhat darker than the adjacent parts. He does not recollect whether the tumour pulsated or not. However, a physician was now consulted, who made an opening into it, (supposing it to be an abscess of some kind) from which coagulated blood was discharged. This was succeeded by a pretty liberal flow of fluid blood, which continued (the physician not being apprized of it) more or less, for fourteen days; when a friend stepping in, arrested the hæmorrhage, by a compress and bandage. The wound healed in a week or two, leaving a depression, extending up and down the arm, two inches and a half in length, and half an inch deep. With the exception of a little stiffness in the shoulder, and parts contiguous to the tumour, the patient now thought the affected arm as sound as the other. He accordingly, began to use it as usual. This, in a short time, induced a return of pain, and swelling, corresponding as before, with the course of the blood vessels, and extending from the axilla to the elbow. The inflammation progressed rapidly, attended with considerable swelling of the arm, and a dark streak, presenting immediately over the tract of the vessels above the elbow. Rest, aperients, and cold applica-

tions gradually subdued the inflammatory symptoms in the space of a fortnight. The arm was again used. Pain and swelling again returned, with high fever, and delirium, the whole of the arm, and even the fingers acquiring, suddenly, an enormous size. A sloughing commenced in the parts, along the dark streak above noticed, whereupon an incision was made five inches long, down to the vessels; from which coagula and blood were discharged. The sloughs were next removed, and the wound suppurated and healed quickly, leaving a narrow furrow down the artery, from the axilla to within an inch or two of the elbow, so that it can now be felt pulsating beneath the finger, through the entire length of the furrow, as distinctly as the radial, or temporal artery. The arm, nevertheless, at this time appears, in all respects, as useful as the other, excepting a slight inconvenience from a stiffness of the shoulder joint; which is most likely a sequel of rheumatism. Mr. W—— relates that palpitation of the heart has been a source of occasional distress to him, through life. He does not think that the hæmorrhages to which he has been subject, have ever manifested a regular periodical character.

By extending my enquiries, I ascertained further, that the grand-father of Mr. W—— was a subject of rheumatism from his childhood. He thereby became much disabled, but in the latter part of his life was less afflicted with it. He was always known to bleed very freely from any wound, however small, that he happened to receive. He was also troubled with frequent ecchymoses, in various parts of the body, epistaxis and hæmaturia; and finally died of hæmatemesis, caused by a fall from a fence, at the age of forty-five.

His only brother was remarkably predisposed to hæmorrhage and rheumatism, and died of the former.

His sisters (six in number) were robust and healthy women; most of them having lived to an advanced age. The mother of Mr. W—— is quite a healthy woman; entirely exempt from any rheumatic, or hæmorrhagic affection.

Mr. W——'s sisters are all healthy. His brothers (eight) have all died; none of them arriving to the age of twenty

years. Most of them were subject to hæmorrhage and rheumatism. So notorious is the predisposition, in the male members of this family, to hæmorrhages, (the father excepted,) that slight wounds or cuts have always been looked upon as dangerous, in whatever part received. Three, out of the eight brothers deceased, died of hæmorrhage. One of them from a cut in the tongue; another from a wound on the cheek; and the third of hæmatemesis.

The nephews of Mr. W—— are likewise much disposed to hæmorrhage, and its concomitant rheumatism. There are among them several cripples; and some have died from loss of blood. On the other hand the nieces are healthy, and not at all inclined to either affection. In short, so far as the history of this truly unfortunate family can be traced, the males have invariably been found to inherit a strong predisposition to the diseases in question; while not a solitary case of the sort has ever been known to occur in any female member of it.

Remarks.—The facts, presented in the preceding narrative, claiming attention, are 1st. The original predisposition. 2nd. The relation and dependance of the two complaints, hæmorrhage and rheumatism. And 3rd. The circumstance of the male members of the family being exclusively affected.

1. The hereditary character of the hæmorrhages, prevalent in the family of Mr. W——, and which obtained among his ancestors in the maternal line, will scarcely be doubted by any, when it is considered that the predisposition thereto has developed itself satisfactorily, in at least three successive generations. With regard to the peculiar morbid condition of the blood vessels, in which the reigning predisposition to hæmorrhage consists, it is needless to speculate at present. Whether it is to be found in a preternatural tenuity of the vessels, original debility of their tunics, or some other morbid state, is difficult to be determined. It is sufficiently evident however, that a weakness from some cause or other, pervades the coats of the circulating vessels. Probably, both arteries and veins are implicated. Circumstances would indicate that

the capillaries are chiefly at fault, as in no instance (excepting that previously given, in which the brachial vein seems to have been ruptured,) has any large blood vessels been found in an aneurismal, varicose, or otherwise diseased state. They appear to possess but little, if any, contracile power. Hence the fact, that hæmorrhage from a lesion of the smallest vessels rarely ceases, without the intervention of art. In confirmation of the atonic nature of these bleedings, it may suffice to remark, that they are neither preceded by, nor attended with unnatural excitement of the heart and arteries.

2. The connection between the hæmorrhage and rheumatism. As a pathological fact this merits particular notice. Are the two affections the result of a common cause, growing equally out of the same morbid condition of the system? Such is the conclusion forced upon the mind by the general history of the case. The coexistence in the human constitution of two separate and independent hereditary diatheses, we conceive to be an impossibility. Gout and phthisis are dependant each upon its own constitutional predisposition. Nor can they be blended together in the same constitution. And so with other hereditary maladies. But it is possible, as in the case before us, for different diseases to originate directly or indirectly, from an identical constitutional defection. Here it is evident, that weakness of the blood vessels lays the foundation for both hæmorrhage and rheumatism; though the order of their occurrence is different; the latter being *consecutive* to the former. Hence, though both are truly speaking referable to the same *remote* cause, their *immediate* production depends upon very different circumstances. The peculiar weakness of the vessels is such as to render them liable to rupture from various causes, as external injuries, immoderate exercise, local determinations, &c. The immediate effect upon the application of these exciting causes, is hæmorrhage to a greater or less extent. The ultimate result is rheumatism in various parts. The correctness of the opinion here advanced, that the rheumatism is consequent to the hæmorrhage, derives additional support from the following

considerations, viz. 1st. That those members of the afflicted family who have bled oftenest, have likewise been most addicted to rheumatism. 2nd. That the ravages of rheumatism have been greatest in those parts of the body, (the inferior extremities) most liable to the influence of the exciting causes of hæmorrhage, viz. local determinations, walking, running, jumping, &c. which have frequently been followed in the case of Mr. W—— by sanguineous extravasations. 3rd. That according to the best accounts, hæmorrhage has generally appeared to be the primary affection. And 4thly. That the violence of rheumatism, has usually abated in proportion to the infrequency of hæmorrhage. This obtained in the case of Mr. W——'s grand-father, who is said not to have bled as often, or suffered as much from rheumatism, in the latter part of his life. The atony of the coats of the blood vessels, generally prevailing among the males of the family in question, most likely induces rheumatism, by the repeated effusions of blood to which it disposes the extreme vessels in the vicinity of the articulations, (the ordinary motions of which may be deemed competent to a lesion of the enfeebled capillaries,) thereby creating obstruction, and consequent inflammation of the parts. The well known fact of inflammation being frequently removed or mitigated by effusion from the vessels of the part may, perhaps, be considered an insuperable objection to this view of the subject. But it should be recollected that the effect of effusion depends entirely upon the circumstances under which it takes place. Thus, when it occurs from the engorged vessels of an inflamed part, immediate relief is obtained; the escape of the effused fluid into the circumjacent parts taking off the previous distension, and facilitating, in the same proportion, the course of the blood. But when effusion of blood takes place repeatedly in a part not inflamed, unless the effused blood is quickly disposed of by the absorbents, it must unquestionably create obstruction in the part, which, by interrupting the natural and uniform action of the vessels, induces an accumulation of blood, an effort on the part of nature to remove which,

constitutes what we term inflammation. We have several times seen considerable inflammation after a lapse of weeks, arise from a rupture of the blood vessels of parts which had sustained no direct injury. We are also informed by Mr. W. that ecchymosis, in whatever part of his body it has shown itself, has usually been attended with more or less pain and soreness.

3. A singular fact, noticed in the foregoing history, and for which, we do not pretend to account, is that the females of the family, are all healthy; while the males invariably inherit the family complaint; the predisposition descending, with equal certainty, through both, to posterity. Here, as in gout, nature displays a mysterious operation of her laws, as it were, but to mock our efforts towards investigation. We can only say, that such facts clearly imply a marked, and radical discrepancy in the original constitutional organization of the sexes.

Art. VI.—*Descriptions of several new species of Shells, and of a new species of Lumbricus.* By Thomas Say, F. M. L. S. *of New Harmony, Indiana.*

1. UNIO *interruptus*, elevated umbonial slope, separating folds of the disk from folds of the anterior margin. Inhabits Louisiana.

Shell transversely oblong-oval or sub-rhomboidal, blackish brown; an elevated rib extends from the apex to the anterior basal angle along the umbonial slope; anterior half of the disk with somewhat oblique undulations extending nearly to the umbonial rib; anterior to the umbonial rib are several arquated undulations, which do not reach the edge of the shell and are slightly cut by an impressed line which is nearly parallel to the rib: *umbo* compressed: *beaks* not elevated, deeply eroded: *posterior margin* very short, rounded: *ligament* margin

rectilinear, the anterior angle elevated, obtuse: *anterior slope* very oblique: *base* rectilinear, parallel to the ligament margin: *anterior basal angle* somewhat rostrated: *cavity of the hinge membranes* (or excavated lunule) fusiform: *within* purplish, iridescent: *cardinal teeth* direct, deeply granulated and lineated over their whole surface: *lateral teeth* entire: *posterior cicatrices* rough, the smaller one orbicular: *anterior edge* blackish.

Length 2 1-5 inches, greatest breadth 3 3-5, convexity 1 3-10 inches.

The species to which this makes the nearest approach, is undoubtedly the *undulatus*, Barnes; but it is more transverse, more oblique anteriorly; the edges of the hinge margin of the anterior slope are not undulated; the umbonial slope is elevated into a distinct separating rib; and it is of a fine livid purple colour within. The eroded surface of the umbones is pearlaceous, iridescent and at the apices it exhibits a dull wax yellow surface. For this interesting species I am indebted to Mr. Joseph Barabino of New-Orleans.

2. Unio *glebulus*, oval orbicular; lateral teeth remote, very short, granulated. Inhabits Louisiana.

Shell transversely oval orbicular, sometimes subovate, very convex, dark brownish: *umbones* not elevated above the general curvature: *beaks* not prominent, (much eroded, exposing a wax yellow surface:) *hinge* and *basal margins* correspondingly arquated; destitute of any prominent angle: *anterior* and *posterior margins* almost equally arquated; the former extending a little with age into a more or less obtuse angle at the extremity of the umbonial slope: *umbonial slope* slightly elevated: *lunule* not dilated: *within* a little tinted with rosaceous: *cardinal teeth* deeply and approximately granulated and sulcated: *lateral teeth* remote from the cardinal teeth, remarkably granulated, their length hardly greater than their distance from the cardinal teeth: *posterior accessory cicatrix* small and approximate.

Length 1 3-5 inches; breadth 2 1-10; convexity 1 3-10 inches.

The transverse simple rotundity of this shell, combined with the much sulcated cardinal teeth, and the remote short

lateral teeth readily distinguish this shell. Several specimens were sent to me by Mr. Barabino.

3. Unio *nexus. Shell* transversely triangular subrhomboidal, much inflated, thick: *beaks* prominent: *anterior side* much depressed, in its middle elevated, so as to make an almost rectilinear hinge margin, with a broad, shallow groove, which extends from the beak to the anterior margin: *anterior margin* forming nearly a right angle with the base, obtusely emarginate in the middle by the termination of the groove: *umbonial slope* carinated, in consequence of the depression of the anterior side: *lunule* very short: *posterior margin* very short, rounded: *colour* light brownish, obsoletely radiated: *within* white: *cardinal teeth* direct: *lateral teeth* rectilinear, short, with but little obliquity: *posterior accessory cicatrix* obliquely elongated, slender.

Length 1 7-10 inches; breadth 2 3-10; convexity 1 7-10.

Closely related to *triangularis*, Barnes; but differs in the greater prominence of the superior portion of the anterior margin; the shorter posterior margin and lunule; the direct cardinal teeth, and the little obliquity of the lateral ones. It was sent to me from Nashville by Dr. Troost. I may remark that several shells of Cumberland river vary much from those of other tributaries of the Ohio.

4. Unio *declivis*, anterior margin rectilinearly and obliquely truncate. Inhabits Louisiana.

Shell transversely moderately thick: *posterior margin* short and obtusely rounded: *beaks* hardly elevated: *hinge margin* rather elevated and compressed, angulated at tip: *anterior margin* rectilinear, descending obliquely forward to a prominent rostrum of the anterior basal margin at the extremity of the umbonial slope: *umbonial slope* not elevated above the curvature of the disk, but is bounded on its anterior side by two slightly impressed lines: *basal margin* arquated: *surface* deeply wrinkled: *within* tinged with purplish: *cardinal teeth* oblique: *lateral teeth* distant from the cardinal teeth, rectilinear, oblique with respect to the base.

Length 1 3-5; breadth; 2 9-10; convexity nearly 1 inch.

Several specimens were sent to me by Mr. Barabino. It resembles *purpureus*, Nob., but in that species the cardinal teeth are direct, and a remarkable feature is exhibited by this shell in its anterior rectilinear declivity, terminating below the anterior middle in a kind of rostrum or prominent angle, which may be compared to that of *nasutus*, Nob., excepting that it is nearly on a line with the base. I have not seen any variety of either of those species that could be mistaken for this. The beaks and umbones in my specimens are very much eroded, exposing a wax-coloured surface.

5. Unio *capillus*, very small, thick, radiated; teeth very thick. Inhabits Wabash river.

Shell transversely suboval, thick, obscure, yellowish, with very numerous dark green radiating lines: *posterior margin* not very short, rounded: *beaks* but little elevated, simple: *lunule* fusiform, very obvious: *hinge margin* and *anterior margin* are little depressed, declining gradually in an arquated line: *anterior basal margin* rounded: *basal margin* nearly rectilinear in the middle: *within* pearly white: *cardinal teeth* direct, thick: *lateral teeth* thick, oblique with respect to the base: *cicatrices* very deep and rounded: *cavity of the beaks* almost obliterated, with a series of small cicatrices extending downwards and backwards.

Length 7-10; breadth 1 1-5; convexity half an inch.

The robust tooth and the thickness of the whole shell have induced many, and myself amongst the number, to consider this shell as the young of *gibbosus*, Barnes; but a very slight examination serves to shew that it is very distinct. The young of that species is always much more elongated transversely, not so thick, with the beaks much undulated, and the series of small cicatrices in the beak cavity is parallel to the hinge margin. As respects magnitude it approaches *parvus*, Barnes, which, however, has the beaks undulated, is rather thin, with oblique small cardinal teeth.

Helicina *occulta*, carinated; carina almost concealed on the spine and nearly obsolete on the body whorl. Found near New Harmony, Indiana.

Whorls about five, carinated or with an acute shoulder, which is almost concealed on the spine by the suture; it becomes more obtuse and almost obsolete or even impressed on the body whorl, but near the labrum it is again very obvious: the whole surface has slightly elevated somewhat regular lines, forming grooves between them across the whorls, and there is an appearance of revolving lines on the body whorl, particularly beneath: *labrum* entire, thick, a little reflected, obtusely a little more prominent towards the base, but not angulated. Greatest breadth less than three tenths of an inch.

All the specimens that I have yet observed are dead and bleached. They occur abundantly in a rugged and abrupt "bluff" a half mile below New Harmony, near the river bank, with many species of Helix, common in many parts of the United States, such as *alternata*, *thyroidus*, *fraterna*, *concava*, Nob. &c. They are much the same size or even a little larger than Helicina (Oligyra) orbiculata, Nob. which species is destitute of carina and of prominent wrinkles or elevated lines; its labrum is reflected but not thickened, with a distinct angle near its base.

Lumbricus *terrenus*, cylindrical, red; segments subequal; sides and venter with four double longitudinal series of short, small, curved bristles, or hooks; back unarmed; clitellus prominent and obvious; posterior extremity not depressed, attenuated like the anterior extremity.

This is our common earth-worm, which has been hitherto considered as the same with the *terrestris* L. of Europe, which however, has the posterior extremity depressed. It differs from L. *leptozonius*, Hagenbach, of Sweden, by having a clitellus. The L. (Hypogœon) *hertina*, Savigny, of North America is described to have a medio-dorsal series of hooks or bristles. The species belongs to Savigny's genus *Enterion*.

REVIEW.

Art. VII.—*A Treatise on Fever, by Southwood Smith, M. D. Physician to the London Fever Hospital.* One vol. 8vo pp. 448. Philadelphia, 1830.

THIS volume is ushered to light, a candidate for public favour, with a sufficient share of "pomp and circumstance." From beginning to end a tone of confidence pervades it, showing clearly that its author sets no common value either on his own powers, or the advantages under which they have been here exercised. And truly, as a plain narrative of pertinent facts, and a lucid exposition of valuable principles, the work possesses no little merit. But whether it be destined to take the place of all antecedent productions on the same subject, and to supersede the necessity of further discussion of the topics it embraces, are points which it might be unsafe to settle in the affirmative. Yet such is the high standing which the tenor of occasional passages in it would seem to claim for it. It certainly professes virtually, if not avowedly, to teach the faculty something which they did not know before. It pretends therefore to originality. Whether it possesses it will be seen hereafter.

The work is put forth as a faithful report of hospital experience. This is no doubt intended to add weight to its character; and, on the minds of many, it will produce the desired effect, in a degree fully as high, as consists with either truth or usefulness. That hospital practice is of great value, as relates to surgery and morbid anatomy, will not be denied. Under no other auspices can those two branches be so profitably cultivated. But, as respects the practice of medicine, considerations of much weight forbid us to make the same concession. All experience testifies, that modes of practice said to be best suited to repositories of the sick in large cities, are not so ap-

plicable to the diseases of other places, more especially of the country. It would be singular and unaccountable, were the case otherwise. In their habits, modes of life, and entire constitutional character, the patients found in hospitals are widely different from those that present themselves to physicians in common practice. Even when labouring under the same diseases, therefore, they require a different treatment. Hence we have always distrusted the judgment or the sincerity of those, who have been loud in their praises of hospital practice as a source of instruction; we mean, in particular, as concerns the treatment of febrile diseases. We must not however dwell on this topic, but proceed to examine the book as it is, and endeavour to speak of it according to its merit.

After a meaning reference to the facilities he enjoys for the study of fever, our author gives a hasty sketch of the most distinguished theories of that complaint, which have prevailed, as well in ancient as in modern times. In this outline there is nothing particularly worthy of notice, except his just rejection of all theories growing out of the notions of humoralism, and his adoption of the doctrine, that fever is necessarily the result of a topical læsion; in other words, that it can have no existence, independently of a "morbid local affection." That most of the eminent physicians of the day are fast uniting in opinion on these two important points of belief, is matter of rejoicing. The opposite hypotheses have already too long and too widely prevailed, whether we have reference to the credit of the profession, or the benefit of man. The many erroneous and destructive schemes of practice that have arisen from them have been a scourge to our race, scarcely lighter than the complaints they were benevolently intended to combat. So true is it, that unsound views in pathology lead inevitably to unsuccessful modes of treatment; and not merely to unsuccessful, but to pernicious ones.

It may perhaps be thought by the reader that we are bound to verify our charge, as relates to the confident and authoritative tone, which we have ascribed to this work. We therefore quote, for the present, the following passage, as a single

specimen out of many we could refer to, of the same description. Others to the same effect will be given hereafter.

That men who exhibit such talent for observation and such acute and active powers of the understanding as many of these authors exemplify in these very works, should, while writing with so much earnestness against each other, fall into one and the same error, and that an error so palpable, is no flattering exhibition of the state of the art of reasoning among the members of the medical profession. The degree in which the science of mind is neglected in our age and country, may it not be justly added? especially in our profession—that science upon the knowledge of which the conduct of every individual mind is so dependent, is truly deplorable.

In his comment on what he deems errors, in the several theories of fever, of which he had spoken, our author, with much seeming truth, derives them all from the following sources; "either that of assuming as a fact what is merely a conjecture; or that of assigning to the genus what belongs to the species; or that of characterizing the disease by what appertains only to a stage; or that of mistaking the effect for the cause." In his adduction of facts to illustrate this, he is happy and satisfactory. His remarks, in opposition to the hypothesis, that "debility" constitutes the "essence of fever," though brief, are important. A more destructive error has never found its way into the science of medicine. And much of the mischief it has done has arisen from the plausible shape, under which it presents itself to those, who know but little of the laws of the human constitution. It is masked under a guise well calculated to attract the inexperienced, and its suggestions are those of seeming wisdom; but a credulous compliance with them leads to ruin. The scheme of practice founded on it has surpassed the sword itself in tormenting its victims, and destroying life. It has committed its ravages chiefly in what is called typhus fever, and in convalescence generally. To the treatment of dyspepsia, dropsy, scrophula and scurvy, as well as to that of many other complaints, the same hypothesis has been destructively applied. Its advocates do not seem to be aware, that different parts of the human body are often in opposite states of excitement, at the same time; that, in reality disease

consists, in a great measure, in the transfer of native strength of action from one tissue or set of organs to another, leaving the former in a state of debility, and giving to the latter a preternatural and morbid amount of vital power. Much less has it been known to these mistaken theorists, that the true cause of debility or deficient action in one portion of the system, is often excessive and even inflammatory action in another portion, possessing higher relations, and essential to life. While therefore they have been labouring, under mistaken views, to restore vigour to the debilitated organs, they have urged the others to a degree of excitement necessarily fatal. Such has been the terrible effect of treating typhus fever after the manner of Cullen and Brown and their followers, more especially those of the latter school. Muscular strength is prostrated in this complaint, and the action of the heart is feeble, because the stomach, brain, and spinal marrow, and perhaps the lungs also, are inflamed. The Brunonian plies his stimulants, and the Cullenian his tonics, to invigorate the energies of the former organs; and what is the issue? Let sound principles unite with sober experience in the reply. In the latter organs a degree of excitement is produced incompatible with life. Inflammation already high is urged to a fatal issue. The same is true of the practice conducted under notions of "debility," in almost every instance, in which it is pursued. In attempting to restore lost strength to one set of organs, as if their symptoms of weakened action were the entire disease, it destroys or incapacitates for their functions another set, of higher standing in the economy of the system. Fortunately for mankind, there is good reason to believe, that the more correct views of pathology, which are now entertained, and which are still further developing and maturing, under the analytical and autopsic method of cultivating medicine, will soon eradicate this fatal practice, or leave it such a mere professional shadow, that it may truly be said of it, *stat nominis umbra.*

Our author's views, in his brief descant on the hypothesis, that the "essence of fever consists in a morbid condition of the *blood*," are worthy of notice. Like every other enlight-

ened physiologist, he considers both the blood and all the other fluids of the body, as the direct product of the action of the solids. From the solids therefore they receive their "condition" and properties, whether "morbid" or healthy, as well as their existence. Whether the case be fever, therefore, or any other form of disease, the action of the solids must be "morbid," before the "condition of the blood" can be so. Hence fever cannot possibly originate in the blood. True; when the action of the solids is deranged in fever, the condition of the blood becomes also unsound, and probably contributes its part, as an auxiliary cause, to augment the general mischief. But the changes, which that fluid sustains in its properties, in different diseases, have never been ascertained. Their existence only is known, not their character, much less their effects in producing further changes in the functions of the solids. Our author indeed informs us that a series of experiments, instituted for the purpose of shedding light on that subject, is now in progress in London, under the direction of a competent conductor. But he frankly acknowledges, that however curious and interesting the results of them may be, he does not believe that they will contribute to the advancement, or in any way subserve the purposes, of practical medicine. In this he thinks correctly. It is through a more thorough knowledge of the solids alone, in their healthy and diseased conditions and relations, that the interests of practical medicine can be promoted. As well might an attempt be made to renovate a decaying vine, by remediate applications to the sickly grape it produces, as to remove any disease from the human body, by acting on the fluids. In either case the attempted process, would be directed backward from effect to cause. But this is in opposition to the course of nature, and must necessarily fail.

Our author manifests a respectable acquaintance with the nature of the inquiry, in which he is engaged, by the question, which he proposes to be answered, in the course of it. They are judiciously conceived, and are as follows.

"1. What is the series of phenomena which constitutes fever? 2. What are the particular phenomena which are common to all its varieties and

combinations? 3. What is the order in which these phenomena occur in the series? 4. What are the organs, and what their states, upon which these phenomena depend? 5. What are the external signs of these internal states, or what are the indications by which their existence may be known? 6. What is the external noxious agent or agents, or the exciting cause or causes of the disease? 7. What is the particular remedy, or the particular combination of remedies which is best adapted to each state of each organ? When these questions can be clearly and perfectly answered, and not till then, we shall know the disease and its treatment."

In the prosecution of his inquiry, Dr. Smith displays a strong attachment to order, and much accuracy of method. This, united to a correct use of language, and a straight-forward manner, removes all occultness and mystery from his investigations, and renders his views exceedingly plain. He rarely expresses an opinion himself or attempts to impose one on his readers, beyond the limits of the evidence he adduces. He regards fever as consisting in a certain morbid condition of certain principal organs of the body, the organs and their condition being indicated by the phenomena called symptoms. These symptoms or external manifestations serve as so many well constructed glasses, to disclose to the intelligent and observing eye the internal state of the parts concerned. He considers it a matter of paramount importance to ascertain what are the organs which are implicated in this disease; "what the conditions are which are induced in them; what organ sustains the first assault; and what organs are attacked in succession." The result of his inquiries on these several points will appear hereafter. In the mean time we deem it justice to him to observe, that the positions he has here laid down, as the basis of his theory, are correct. Fever does consist in a morbid affection produced by its causes in some of the leading organs of the body. And that affection is originally local. From the spot first injured, as a centre, the mischief extends, by constitutional laws, to other important organs, until the derangement becomes general. We shall only so far anticipate the views of our author as to observe, that the ruling organs of the body, which control its actions, both in health and disease, and *make it what it is*, are those contained in the three great cavities, the

abdomen, the thorax, and the cranium; in language more circumstantial, they are, the chylopoetic organs generally, the lungs and heart, and the brain with its appendage the spinal cord. The nerves derive their importance from their connexion with the two latter organs. Fever begins in one or another of these sets of organs, usually we believe in the chylopoetic, and extends to the others, by sympathy, until the whole become involved. For in every case of fever they are all implicated. While either of them is sound, it is impossible for that complaint to have a lodgement in the system. The reason is plain. So powerful and controlling are these organs, that if one set of them is in vigorous health, it maintains the health of the other parts of the body. So intimately however are they associated, and so great is the influence of each over the others, that a serious læsion produced in any one of them, usually extends its effects to the whole. It will appear hereafter that the converse of this proposition is also true.

In his second chapter our author proceeds to identify fever, or show what it is. In all febrile complaints he finds some things in common, and others exceedingly different. In no two countries, especially if remote from each other, are fevers alike. In the same country, the fever of one season differs materially from that of another; and even in the same season and country, fever differs in different individuals. Still there is something present, which so completely identifies the complaint, under every variety it assumes, that no doubt or difference of opinion prevails, as to its existence. In every instance it is acknowledged to be fever. Bring into the ward of a hospital three persons, one labouring under the common fever of Great Britain, another under yellow fever, and the third under plague. The aspects of the three patients will differ from each other as widely, as imagination can well conceive. But no matter. From the positive form and essential characteristics, under which fever presents itself in each case, its existence is promptly acknowledged in all. This arises from something common to the complaint, as a genus, or in the abstract; and physicians have found it difficult to determine what it is. In attempting

this analysis, Boerhaave collected, with much labour, all the symptoms belonging to every variety of fever, and found the aggregate unexpectedly great. But, after striking off those which he had not uniformly seen in every case of the disease, he was surprised to find the catalogue reduced to three; "shivering, a frequent pulse, and preternatural heat." Yet, in many cases of fever, of the most severe and fatal description, neither of these is present. In their attempts to make up the common character, or what they considered the "essence" of the complaint, Cullen, Wilson Philip, and other physicians have been equally unsuccessful.

In his remarks on this subject, our author divests himself of the character of an ontologist. He denies fever to be an entity, or abstract being, possessing a peculiar nature. It is a series of events; and the object of his inquiry is to discover what those events are; in particular, what the events are which invariably concur in the series; and in what order they constantly succeed each other. And he alleges, that when this shall have been discovered, all will be ascertained that we can ever know of what is termed the nature of fever; because nothing more is known of the nature of any process.

Where, then, he inquires, shall we look for the events sought after? Not in the symptoms. They are not events, but the signs or indications of them. Symptoms depend upon the states of organs; they are the external and visible signs of internal, and, for the most part, as long as life continues, invisible conditions. It is to the state of the organs that we must look for the events of which we are in search.

"Are there any states of any organs that always exist in fever? Are the states constant? Are the organs affected constant; and can both be ascertained? If this can be truly answered in the affirmative; if it can be proved that there are certain conditions of certain organs which invariably exist in fever, in every type, in every degree, in every stage of it, we shall have arrived at a satisfactory conclusion relative to the first part of our inquiry.

"The evidence is as complete as observation during life and inspection after death can make it, that a morbid change does take place in a certain number of organs in every case of fever, from the most trivial intermit-

tent to the most alarming continued fever, from the mildest plague to the most malignant typhus: that at the two extremes of this scale, and at all the intermediate gradations of it, there are certain organs which are always affected, and that the affection in all is similar.

"The identity of the organs is inferred from the indications they give of disordered function during life: the identity of the affection is inferred from the similarity of morbid appearances which they exhibit on examination after death.

"The organs affected are those which constitute the nervous system; those which constitute the circulating system, and those which constitute the systems of secretion and excretion. The spinal cord and the brain; the heart and the arteries, especially their capillary extremities; the secreting and the excreting organs, which in fact are composed, essentially, of the capillary extremities of the arteries; the secreting and the excreting extremities of these arteries, especially as they terminate in the external skin, and in the mucous membranes, which form the internal skin, this is the chain of diseased organs: derangement in the nervous and sensorial functions: derangement in the circulating function: derangement in the secretory and excretory functions, this is the circle of morbid actions.

"There never was a case of fever in which all these organs and affections were not more or less in a morbid state: there never was a concurrence of this morbid state, in this complete circle of organs, without fever. The events which *invariably* concur in fever, then, are a certain deviation from the healthy state in the nervous and the sensorial functions; a certain deviation from the healthy state in the circulating function; a certain deviation from the healthy state in the functions of secretion and excretion. A deviation from the healthy state in one circle of actions will not present the phenomena of fever; a deviation from the healthy state in two circles of action will not present the phenomena of fever: there must be a deviation in the three circles before fever can exist. Such then are the common phenomena of fever."

Our author proceeds:

"But it is not the invariable concurrence of a particular number of events that is alone sufficient to constitute fever: to this must be added invariableness of concurrence in a particular order. As will be shown in the proper place, there is complete and irresistible evidence that these events do occur in one invariable order. Derangement in the functions of secretion and excretion never comes first in the series: derangement in the nervous and sensorial functions never comes last in the series: derange-

ment in the function of the circulation never comes either the first or the last in the series, but is always the second in succession.

"The order of events then is first, derangement in the nervous and sensorial functions; this is the invariable antecedent: secondly, derangement in the circulating function; this is the invariable sequent: and thirdly, derangement in the secreting and excreting functions; this is the last result in the succession of morbid changes."

Such is the Doctor's picture of fever; and whether correct in every point or not, he is so far clear and intelligible in his delineation. But he now enters on an enterprise, in which his success is much more questionable. It is to prove the existence of a radical difference between fever and inflammation; a difference not merely in their phenomena but in their essence. He acknowledges that the two affections possess many things in common. In the circulatory and secretory functions the derangement is the same. But not so in the nervous and sensorial functions. The derangement which takes place there "*apparently* different in kind," is pronounced certainly and invariably different in the order of its occurrence. In pneumonia, in enteritis, in hepatitis, the spinal cord and brain are *never* the organs in which the first indications of disease appear: the earliest indications of disease that can be discovered have their seat in the affected organ itself; it is only after the disease has made some progress that other organs and functions are involved; and, apparently, the last to be involved, and certainly the least to suffer, is the nervous system!

These views appear to us unfounded. Whether it occur in the "Febres" or the "Phlegmasiæ" of Cullen, or in any other form of disease, we believe inflammation to be essentially the same, provided it be seated in the same kind of tissue. The only appreciable difference is in the degree; and that depends on sundry causes, some of them at times not easily detected. The most powerful of them would seem to be, temperament, constitutional diathesis for the time being, and the kind and degree of original irritation. We say "original irritation;" for it is from an affection of that kind alone that inflammation can arise; and we need scarcely add, that irritation has its seat chiefly in the nervous and cerebral system. Of no other

system is that affection so predicable. This is to be regarded as a pathological axiom. Solicitous to be clearly understood on this subject, we shall pursue the discussion of it a little further.

Every primary irritation is necessarily local, and is produced on some organic tissue, as a whole, made up of all its subordinate tissues. Of these subordinates, the nerves constitute a leading one. Is the organic tissue, on which the irritating impression is made, the skin, or the mucous lining of the lungs or the alimentary canal? and is the impression powerful? The nerves of the part convey it to the brain, that organ is deranged by it, and, from its universal influence, the entire system participates in the mischief. Is the irritating impression slight? no real inflammation follows. Is it deep and severe? Inflammation is produced. Is it powerful in the highest degree? Life is either suddenly extinguished, or its energies are so prostrated that reaction cannot take place, and again the affection produced is not inflammatory. But it is much worse. It is usually termed congestive; but, from the debility, diminished sensibility, and inaction, which characterize it, it might be not inaptly termed *paralytic*. It exists in the worst cases of plague, yellow fever, epidemic cholera, scarlatina, and typhus.

This statement is equally true of every irritating impression, whether it be made by a febrific miasm, any other poisonous agent, caloric, or mechanical violence. Is the stomach deeply injured, by a severe blow or a wound? The irritation or shock is communicated through the nerves to the cerebral system, as clearly appears from the debility, confusion, insensibility, or other form of intellectual derangement produced. If the powers of life be not depressed below the point of reaction, inflammation, accompanied by fever, perhaps malignant, ensues. But if the system cannot react, the fever will be congestive, and still more malignant. Here the order of events is precisely the same with that which occurs in yellow or any other form of bilious fever. In the latter complaints the poison, as we believe, makes its first impression on the mucous lining of the stomach, with which it comes into contact, but never substan-

tially goes beyond it. Through the nerves of the stomach it throws its influence on the brain, irritates or shocks that organ, deranges its functions, and thus depresses, for the time, the powers of the whole system. Does strong reaction take place? gastric inflammation is the issue, accompanied by a fever corresponding in character. Are the vital powers so far depressed by the original impression, that very feeble reaction, or none at all, occurs? The product is congestive fever. To illustrate this point still further, let the original irritation be a severe burn, on the surface of the body. Every symptom clearly indicates an affection of the brain. The irritation or shock is conveyed to that organ by the nerves of the skin, with which the caloric had come into contact. If the læsion be not too great, inflammation follows; if it be, congestive fever occurs, or death in a few hours. If the fever arising from an external burn is not, in all respects, similar to bilious fever, much of the difference is ascribable to the different condition of the vital properties of the skin and the stomach, and their different sympathies with the other parts of the body. By pursuing this discussion it might be easily made appear, that, in every case of severe inflammation, the cerebral system is previously irritated and deranged; not indeed primarily; that cannot be; but secondarily, through the nerves of the part originally irritated. If, in alleging that the "nervous and sensorial functions" are *first* deranged in fever, our author mean that the brain, spinal cord, and the nerves generally are *primarily* acted on and injured by the febrile miasm, he is mistaken. The poison cannot primarily and immediately reach those parts. It can only throw its effects on them secondarily, through the medium of the nerves of the part, which it does reach. This is true of every irritating agent, except moral and intellectual ones, and mechanical violence inflicted on the head. They act on the brain primarily, and, by deranging it, injure other parts by sympathy. We conceive then that Dr. Smith has failed in his effort to point out an essential difference between inflammation and fever. Nor does a reference to practice strengthen his hypothesis. Making the necessary

allowance for its extent and degree, and also for the constitutional diathesis at the time, the same mode of treatment will always remove inflammation, no matter whether it be called "febris" or "phlegmasia," provided it be seated in the same organs. The general antiphlogistic practice, with such local means as the particular seat of the affection may require, will perform the cure. Let the sensorial system be as violently and extensively injured by the working of passion, or by mechanical violence, as it is by miasm in typhus fever, and the inflammation that will ensue will be equally as deep and difficult to remove. Each case will be marked by the same general prostration of vital energy, and the same degree of mental derangement.

Our author proceeds to describe the different forms which fever puts on, according as it is seated more immediately in the sensorial, circulatory, or secretory system. In this effort we think him successful. He is also happy and instructive in the view he gives of the boundless variety of character which this complaint derives from different degrees of intensity, and different combinations of topical læsions. Had he however directed his attention more to the chylopoetic organs, as a seat of fever, his delineations would have accorded better with the forms of that complaint in the United States, especially in the middle and southern sections of them. True; he includes those organs in what he denominates the "febrile circle;" but he does not ascribe to them, in its full extent, that important relation to the other parts of the system, which they certainly maintain. We recommend however to the reader his exposition of the common seat, pathology, and varieties of fever, as a specimen of sound doctrine, and excellent analysis, and as leading to important practical results.

Our author justly complains of the divisions and nomenclature of fever, contained in systems of nosology, as an arrangement altogether artificial and arbitrary, and generally without any definite meaning. He might have added, that they have often an erroneous meaning, which misleads the unreflecting portion of the profession. He considers every febrile nomen-

clature imperfect, which does not bestow on each form of the complaint, a name indicative of the organs and their condition, on which it depends. On this point his opinion is correct; and it is much to be regretted that the present state of medical science does not admit of the perfection he requires. Ages we fear must pass away, before the nomenclature of medicine can be so improved, that every name will disclose the character of the disease it designates. He objects to the division of fevers into idiopathic and symptomatic, on the ground that the symptomatic are not fevers but inflammations. He does not therefore believe in the existence of symptomatic fevers. Here again we think him wrong. It is idiopathic fever that has no existence. Every form of fever, even according to our author's own statements on the subject, results from some primary topical læsion, and is therefore symptomatic. Yellow fever is as palpably the product of a preceding local injury, as the fever which arises from a gunshot wound.

Our author is also dissatisfied with the division of idiopathic fevers into intermitting, remitting, and continued, and pronounces the classification "in the highest degree trivial." Nor does he object less to the division of continued fevers into synocha, synochus, and typhus. He declares that, according to its nosological definition, synocha is identical with "acute inflammation." And, as to the synochus and typhus of nosology, he asserts that they are mere creatures of fancy, and have no real existence in nature. On these points we confess we do not feel impelled by any very cogent reasons to differ widely from him. That the language and arangement of nosology are antiquated, loose, and often unmeaning, not to say positively barbarous, cannot be denied. Like every thing else, deriving its origin from times of ignorance, they require a thorough reformation, if not a revolution. We are not, however, prepared to say, that the present state of medical science qualifies us to make the amendment desired. A more thorough and accurate knowledge of diseases than we yet possess is necessary to enable us to designate the nature and character of each, by the name bestowed on it. We hold it

doubtful whether the change in nomenclature attempted by Broussais will contribute to the benefit of his system, or the permanency of his fame. Further inquiry may convince even himself that the effort has been premature.

Dr. Smith pushes his complaints against nosology still further, and alleges that scarlatina, measles, small pox, and all the other exanthemata of Cullen are genuine fevers, and should be so considered and classed. This opinion we have ourselves long entertained, and hundreds of times expressed, and feel convinced that it will become universal. We doubt whether the great Professor of Edinburgh did not propagate as much error in medicine, by his system of nosology, as he did of truth by his other writings. He was perhaps the most thorough-bred and stubbornly devoted theorist that ever figured in the profession, Boerhaave and Galen not excepted. His influence in medicine was astonishing; yet it is still more astonishing that it should have been so. No writer or teacher was ever less original. He did up his entire work, great as it was, with matter furnished by others, attaching to it his own interpretation. We do not recollect an original thought in all his writings. Whatever of novelty they contain is theoretical. The following passages of our author contain much good sense and sound judgment on the dividing and naming of febrile diseases.

"The more we investigate the subject, the more satisfied we shall become that continued fever is one disease and only one, however varied, or even opposite, the aspect it may present; but that it differs in intensity in every different case, and that this and this alone is the cause of the different forms it assumes. Many of these diversities it would be frivolous to distinguish: some of them, on the other hand, it is of the highest importance to discriminate. For all useful and practical purposes, it is necessary only to arrange the different assemblages of symptoms into two great classes, the one comprehending the mild and the other the severe forms of the disease. All the forms that continued fever can assume, and all the individual cases that can occur under either, must be mild or severe, and, therefore, must readily find its place under one or other of these divisions. The only real difference in the disease being a difference in degree, it is proper that the principle of the division, by which the varieties it presents are classified, should be founded on this, the only true distinction of which it admits.

"It is difficult to frame, and still more difficult to bring into use, new terms; and there is nearly equal inconvenience in using old terms in a new sense; but if the new meaning affixed to an old term be clearly intimated and rigidly adhered to, it is, perhaps, upon the whole, productive of less evil to adopt the old, thus determining and limiting the signification, than to propose a nomenclature entirely novel. For this reason, and only for this reason, I propose to adopt two words, borrowed from the nosology of Cullen, and in common use. These words are here employed merely to express differences of degree relative to one and the same disease. The mild degree may be denoted by the term synochus: throughout this work, this term will be used to express the milder form of fever; that is, its ordinary or common form, or that which it is found most frequently to present in this metropolis, and, I may add, in this country. The severer form, on the other hand, may be designated by the term typhus. Each will be found to present a distinct assemblage of symptoms; each will be found to depend upon a particular condition of certain organs; each will be found to require a peculiar treatment.

"For the purpose of distinguishing further important differences, that is, differences which bear an important relation to practice, it will be convenient to divide each of these two great classes into two minor sections. Thus, synochus may be divided into synochus mitior and synochus gravior; and typhus into typhus mitior and typhus gravior. This will afford convenient and ample means of throwing into distinct groups all the varieties of fever that occur in this country, which it can be of any practical importance to distinguish."

The arrangement here laid down has an immediate and important bearing on practice, and may be made to embrace every febrile affection, whether intermitting, remitting, or continued, and whether simple, eruptive, or exanthematous.

From the meaning here affixed to the terms, every case of fever is, according to its degree of intensity, synochus mitior or synochus gravior, typhus mitior or typhus gravior. That the treatment moreover may be adapted to the disease, its vigour and general character must be governed by the same principle. It also must be mitior or gravior, according to the affection it is intended to remove. This division of fever, therefore, is founded on the actual condition of the organs included in the "febrile circle," and so far indicates the practice to be pursued. Notwithstanding however the approbation here expressed, we prefer the division of fever into *simple*, (by which

we mean that of mere excitement) *inflammatory*, and *congestive*. This arrangement is more simple and accurate, conforms equally to the condition of the system, and is more definite in the practice it suggests.

In chap. III our author treats of synochus, in its grades of mitior and gravior, acknowledging that he intends his remarks to apply only to the common fever of England, under those modifications. His analysis of the complaint is minute, setting forth what he considers its component parts, in the order of their occurrence, from its commencement to its maturity, and from that to its termination. He states the earliest symptom to be "a loss of mental energy, consisting particularly in indistinctness and consequent confusion in the train of ideas; in inability to attend to their relations; and, as a necessary result, in the loss of power to think clearly." Closely connected with this is the "loss of energy in the voluntary muscles. Lassitude is the result." Such is the feebleness of the patient, that he can neither move with vigour, nor even sit without weariness.

"The next symptom in the order of succession is still more characteristic: it consists in an uneasy sensation which is quite peculiar to this state of the system." Such is the wretchedness of the sick, under this feeling and the uneasiness which accompanies it, that they would gladly "exchange it for intense pain."

The symptom which next succeeds, in the series, is "positive pain," usually "first felt in the back or loins, and then in the limbs."

Already the countenance has experienced a striking change. "Its expression is that of dejection." The face is pallid, and the features shrunken. The skin becomes preternaturally sensitive to vicissitudes in the atmosphere, especially in its temperature from heat to cold. "Chilliness is felt even in a heated room, or in a warm bed." This often increases to "shivering." In the mean time the skin possesses its natural temperature, and is sometimes preternaturally hot. There is no diminution therefore in the heat-producing process. The chilliness arises from the morbid sensibility of the cutaneous nerves.

As yet no affection of any other organ is very obviously developed. The circulating system is just beginning to feel the disease. The pulse is "more languid than in the state of health; sometimes it is also quicker; at other times it is slower; now and then it is scarcely changed in frequency, but its action is invariably weaker than in its sound state."

Respiration sustains a simultaneous change. It becomes "shorter and quicker than natural; the chest does not expand so freely, and compensation seems to be sought in an additional number of respirations." When the patient remains in a state of inaction, the circulatory and respiratory processes are, in many cases, but little disturbed; but the gentlest exercise hurries both in a very high degree.

To this stage of the complaint succeeds that of exacerbation. The pulse, from having been feeble, becomes "more full and strong, and generally more quick than in a sound state; and the skin, which was cold, becomes preternaturally hot. It is at this period that derangement takes place in the "functions of secretion and excretion. The mouth is now dry and parched; the tongue begins to be covered with fur; thirst comes on; and the secretion of the liver," and indeed of all the other abdominal viscera, as well as of the kidneys, becomes deranged. The altered appearance of the evacuations testifies to this.

The fever being now "fully formed, the series of morbid phenomena is completed; any thing more that happens is referable to degree and to duration," and cannot be derived from any other source. "We now see," says our author, "that the organs affected, constitute precisely that system of organs which has been described as forming the febrile circle; that the symptoms, which denote the fever, are just the symptoms which indicate derangement in the several functions performed by these organs; and that the order in which they become successively involved is exactly that which has been assigned."

The Doctor alleges that now is the period of the accession of headach, and not at the very beginning of the complaint, as it is asserted by Dr. Clutterbuck. That affection does not constitute the *first* symptom of fever. The disease therefore

does not originate in inflammation of the brain. In this stage of the complaint the sensorial functions are still more strikingly affected. Reason and judgment are usually more enfeebled and confused; yet sometimes the reverse is true. The intellectual powers are more acute than in health. This is more especially the case with the functions of vision and hearing. The same is frequently true of cutaneous sensation. But, owing to the altered condition of the membranes, on which the gustatory and the olfactory nerves are distributed, the "senses of taste and smell are nearly obliterated." Another distressing symptom of this period is, that sleep, which had been broken and unrefreshing from the onset of the disease, is now almost entirely denied. The beats of the pulse are usually from 90 to 100, rarely exceeding the latter number, and sometimes not rising above 80. They are also fuller and stronger than in health.

Our author states that the ordinary fever of London is materially different now from that which prevailed in the metropolis two centuries ago. It is much less marked by intermissions and remissions. Owing to causes therefore which he considers obvious, the complaint, within that period, has been strikingly changed. When mild, the disease terminates about the end of the second week, and the patient becomes convalescent. But severe cases are protracted to the fourth, and, at times, even to the fifth week. That term they rarely exceed.

It would be doing injustice to our author to offer the foregoing as a full representation of the picture he has drawn of synochus mitior. It is nothing but a sketch of the course he has pursued in his ample delineation of it. Those who wish to possess a competent acquaintance with it, and to derive from it all the knowledge it is calculated to impart, can attain their end only by a careful examination of it in the original work.

To the same source we must refer for the interesting details, which the volume contains, on synochus gravior, in its various modifications. These are given under distinct headings, which

serve of themselves to throw some light on the matter of the sections to which they are prefixed. They are as follows.

"1. Synochus gravior with cerebral affection.
" ——— Subacute cerebral affection.
" ——— Acute cerebral affection.
"2. Synochus gravior with thoracic affection.
"3. Synochus gravior with abdominal affection.
"4. Synochus gravior with mixed affection."

It will be readily understood that in the three first varieties the disease has its stronghold severally, in the brain, the affection being subacute or acute inflammation, in the viscera of the thorax, and in those of the abdomen; and that, in the fourth, the contents of the three great cavities are equally assailed. The fourth variety is, of course, the most formidable, every vital organ of the body being attacked. The spinal cord usually suffers in common with the brain, and nearly in the same degree. Of mixed cases the writer speaks in utter despair. "These are just the cases which bid defiance to the most skilful and vigorous measures, which the medical art can employ to control them; which seize upon their victim with a force which no human agency can resist or counteract; which in malignant epidemics destroy life in a few hours or in a single hour, and in ordinary seasons in a few days."

As matter to illustrate and confirm his views, on all the several points he has discussed, the Doctor gives reports of numerous cases, which had undergone *post mortem* inspection in the London Fever hospital. In the estimation of the public this will doubtless add much to the authenticity of his opinions, and to the value of his work.

Chapter IV treats of typhus mitior and gravior, under varieties analogous to those of synochus. The two diseases indeed are represented, both in description and express assertion, as different grades of the same fever. "The difference between these two diseases arises entirely from a difference in intensity." Yet "the appearance of a person labouring under typhus is so different from that of a person labouring under synochus, that no one ignorant of the disease, who saw these

two persons, for the first time, would believe that both were afflicted with one and the same malady." To the enlightened pathologist, and indeed to every physician of experience, this is perfectly intelligible. What two forms of disease can be more dissimilar than scarlatina simplex and scarlatina congestiva? Yet they are but different grades of the same complaint.

Although the symptoms which denote typhus are strikingly similar to those which characterize synochus, yet they appear under certain modifications, the nature and ground of which, as well as the practical inferences deducible from them, our author thinks have been generally misunderstood. He deems it important to correct the mistake. In this we concur with him. The following he specifies, as some of the most prominent points of difference between these two forms of disease.

"1. The prostration of strength, both physical and mental, is greater in typhus than it is in synochus.

"2. The chilliness is, upon the whole, greater and of longer continuance than in synochus; yet there is less constantly shivering, and the heat, when it succeeds this state of chilliness, is seldom as great as in the latter.

"3. The febrile uneasiness is greater; the restlessness is incessant; the face is pallid; the features are shrunk; the expression of the countenance is most peculiar, being strikingly indicative of weakness and suffering.

"4. There are cases in which the pain of the head is equally severe as in synochus; but this may be considered as rare. In general it is less acute. Dullness, confusion, stupor, giddiness, are more common than severe pain, and are often the substitutes for it.

"5. When pain is present, it diminishes sooner and disappears more compleatly than in synochus: when it is not present, the advancement of the disease is indicated by increasing insensibility, and by the rapid transition of dullness or confusion into a state of stupor approaching to coma.

"6. There is little or no sleep; * * * there may be no violence; but there is abundance of inquietude.

"7. Delirium is more constantly present than in synochus; and when it comes it comes earlier. It consists moreover of low muttering incoherence rather than of loud and violent talkativeness; and is expressed in moaning rather than screaming.

"8. * * * * Like delirium, muscular tremor is much more constantly present in typhus than in synochus; and its relation to delirium is so

close that it is sometimes observed to supervene on the same day."

The points of difference marked 9, 10, 11, 12, and 13, are less striking. We therefore omit them.

"14. Typhus terminates much earlier, whether favourably or unfavourably, than synochus."

The preceding are the distinctive characteristics of typhus mitior. On typhus gravior the volume speaks as follows.

"All the examples of this form of fever are referable to two classes; one in which the arterial action is excessive; and the other in which it is oppressed, or rather overwhelmed.

"1. In the first, the patient lies insensible, with delirium, perhaps so violent that he cannot be kept in bed without restraint; with extreme restlessness and constant watchfulness; with rapid and panting respiration; with a tender abdomen, perhaps with frequent and involuntary stools, a dry, black, and hard tongue, a quick, yet weak pulse, and the skin universally and pungently hot.

"2. In the second, he lies insensible, with a cold and dusky skin; with a swollen and livid countenance; with a heavy and oppressed respiration; with a pulse perhaps not to be felt, or, if distinguishable, either so rapid that it cannot be counted, so small that it is like a thread beneath the finger, and so weak that it is lost by the slightest pressure, or else slow, irregular, and intermittent. In this state the patient is almost as completely paralyzed as in apoplexy, and the attack is almost as rapidly fatal as apoplexy. It constitutes what has been called congestive fever."

On the pathology of this form of fever, the following very interesting remarks are subjoined.

"No morbid appearances are visible in the organs which seem capable of accounting for death. There are signs of vascularity; the vessels are turgid with blood, and consequently the organs on which they are spent are in a state of congestion. But they seldom if ever exhibit any real appearance of inflammation, and still less do they contain any true inflammatory product. Why? Not on account of debility; but because the force of the disease is so great as to overwhelm the powers of life at the first onset, allowing even of no reaction, and much less of that continued excitement which is part and parcel of the real inflammatory state, and which is indispensable to an inflammatory product. Reduce the intensity of the disease a little, bring it just within the limit that is compatible with the continuance of life for a given time, and then the products of inflammation at once appear in the greatest possible purity, variety, and extent."

Chapter V treats of scarlatina, and is short. We pass it

by without analysis or further remark, not because it wants merit, but because we want space. The reader will peruse it with advantage.

Chapter VI, in which the pathology of fever is considered, is very long. It is composed chiefly of diaries of cases, and reports of appearances on dissection. The cases we think unnecessarily multiplied. They amount, in all, to 110. One third of the number might have been rendered equally instructive (under proper management much more so) and the examination of them would not have so completely wearied the reader. All cases not essential to the illustration and settling of principles and practice, are mere book-making expletives. A superabundance of them, in discussion, resembles an excessive crop of leaves on fruit-trees, which overshadows the fruit, deprives it of nourishment, and renders it at once dwarfish and crude.

Still however the chapter contains much valuable matter. The cases are arranged according to the writer's division of fever, and are calculated to illustrate the pathology of the complaint, in its several varieties of cerebral, thoracic, abdominal, and mixed. In other words, they demonstrate, by autopsic evidence, the morbid condition of the brain and spinal marrow, the lungs and heart, and the abdominal viscera, of those who suffer from the several forms of the fever of England. We say the "fever of England;" for it will appear hereafter that the writer's remarks do not apply so fully to the pathology of the fevers of our own country. The external appearance of the body, after death, is also circumstantially described.

In addition to its cases, the chapter contains many sound and pertinent remarks, entirely creditable to Dr. Smith, as a pathologist. We shall extract a few of them, which will be specimens at once of style and opinion.

"In proportion as our knowledge becomes perfect, we are thus enabled, during life, and at the bed-side of the patient, to see what is going on within his brain, within his lungs, and within his intestines, with as much distinctness and certainty as we could were the cases in which these or-

gans are enclosed, and the organs themselves transparent. The highly interesting and important fact demonstrated by the examination, in the manner of which we have just spoken, of large numbers of fever patients is, that the changes which take place in the organs are uniform; that the symptoms by which these changes are denoted are likewise uniform, and therefore, that it is possible to arrive at a perfect knowledge of the phenomena of fever."

"The pathology of fever comprehends the morbid changes that take place in the solids and the fluids of the body. It is probable that the changes in the fluids are wholly dependent upon those which take place in the solids, although the vitiation of the former must necessarily react upon, and increase the derangement of the latter. If it be true, as is highly probable, that the changes in the solids are beyond all comparison of the greatest importance, as not only antecedents, but *invariable* antecedents, or causes, it may be considered fortunate that our knowledge of their diseases is so much more advanced than our knowledge of the diseases of the humours. The morbid changes of the solids are ascertained with a great degree of exactness, it may almost be said with a great degree of perfection; while those which occur in the fluids are almost wholly unknown."

For a further, and competent knowledge of this long chapter, the reader is referred to the work which contains it.

Chapter VII is headed "*Of the Relation between the Phenomena of Fever; or the Theory of Fever*," and will be read with much interest, by those who are pleased with theoretical discussions. Our author gives the following, as his exposition of the theory of fever.

"In the preceding chapters it has been shown what are the real events which take place in fever, the assemblage of which constitutes the disease: it has also been shown in what order these events succeed each other, and upon what conditions of what organs they depend. To assign further the true relation between these events, is to establish the theory of fever in the only philosophical sense of the term theory: and that relation must already have suggested itself to the mind of the attentive reader."

From what he has already seen the reader must be prepared to believe, that, Dr. Smith's theory of fever differs materially from that of Broussais. A few further quotations will show wherein that difference consists.

"We have seen that the first indications of disease are clearly traced

ble to the nervous system: that the disorder of the functions of the brain and spinal cord with which the attack always commences, demonstrates that these organs form the primary seats of the malady: that the derangement in the functions of these organs is truly *invariable*, and is invariably the first morbid condition that is observed to take place: that there never was a case of fever, from the slightest to the most severe, in which these organs were not in a greater or less degree in a disordered state, and in which that disordered state did not precede every other. This affection of the nervous system then, the invariable antecedent of all that follows, is the primary essential event in the morbid series which constitutes fever.

"What the real nature of this primary affection of the nervous system is, we are wholly ignorant, and we ought at once to confess our ignorance. We have already entered into some considerations, derived from the difference in the order in which the phenomena of fever and inflammation succeed each other, to show that these two diseases are not identical.* When these phenomena are still more attentively considered, other differences are observable between them, which confirm the opinion that the two diseases are not the same. Not only is derangement in the nervous and the sensorial functions invariably the first in the series of morbid events in fever, while it is not the first in inflammation, but that derangement is always much greater in the former than in the latter, and proceeds in a regular and determinate course, such as has been fully explained in the preceding pages, and to which there is nothing analogous in the progress of inflammation.

"To the condition of inflammation a peculiar but an unknown condition of the blood-vessels appears to be indispensable. To the state of fever, no such condition of any part of the vascular system, as far as we have the means of judging, is absolutely indispensable, although it be very commonly coincident. No such condition appears to be present, at least no such condition has yet been ascertained to be present, either in the very mildest or in the severest form of the disease: at the latter extreme of the scale, at least, we might expect to find the most striking and unequivocal indications of the existence and operation of inflammation, were that agent really present, and yet it is precisely here that the ordinary signs of inflammatory action are completely absent.

Moreover, we have no example of instantaneous death by the sudden excitement of inflammation in any organ, or in any number of organs: inflammation is a process: a certain number of events take place in a certain order; and there is always, as far as has been hitherto observed, some interval between these events. A case is recorded in which inflam-

*See page 63, et seq.

mation of the bowels (acute enteritis) proved fatal, as was supposed, in eight hours from the commencement of the attack; but so rapid was the process, that the intelligent surgeons who witnessed it doubted whether the time when the disease began could have been noted accurately: at all events, it does not accord with the best established fact relative to the process of inflammation, that it should prove fatal without the lapse of some hours. Fever, on the contrary, does not need as much as a single hour to complete the work of death. It is well known that the poison which, in a certain state of concentration, produces fever with the ordinary period of duration, in a higher state of concentration produces instantaneous death; and that, in certain climates and seasons, it is not uncommon for persons previously in sound and vigorous health, on exposure to that poison, to sicken and to die in a shorter space of time than is requisite, under ordinary circumstances, for the mere formation of the inflammatory process. The state of the system, in the primary attack of fever, and the state of the system in inflammation, do not, therefore, appear to be identical. The truth is, that we do not know what the real state of the system is in either case, but we see that the phenomena of the one differ from those of the other; to conclude, therefore, that the states are the same is not a sound induction. While, then, we are constrained to admit that we know nothing of the nature of the primary affection of the nervous system in fever, the closest consideration of all the phenomena alike constrains us to conclude, that that affection is peculiar and specific."

The Doctor acknowledges, however, that there is inflammation in fever, but is unwilling to admit that it is true, pure, and simple inflammation. He thinks it "inflammation and something else." But we must again permit him to state his own opinion, lest we should fail to do it accurately.

"With two apparent exceptions, (whether these two cases form real exceptions may still admit of doubt) the vascular derangement connected with, and dependent upon nervous derangement, passes sooner or later into true inflammation. Of this we have the most complete and indubitable evidence—evidence derived both from changes, the known results of inflammatory action, produced in the structure of organs; and from the generation of new products, such as are formed by no other known process but that of inflammation. Almost every change of organic structure which inflammation is ascertained to be capable of producing, is found to take place in fever: almost every product which inflammation is ascertained to be capable of forming, is observed to be generated in fever: it is not possible to doubt, therefore, that the morbid condition

into which the vascular system is brought in the progress of fever, is that of inflammation. In what circle of organs inflammation is peculiarly liable to be excited in this disease, by what particular character febrile inflammation is distinguished, and what remarkable differences it exhibits in intensity and extent, have been fully illustrated.

"It follows, then, that the second event that takes place in the morbid series constituting fever, is inflammation.

"But however really and constantly inflammation may take place in fever, and in whatever intensity, and to whatever extent it may be carried, yet the inflammation is never pure or simple. the condition of the inflamed organs is never the same as that into which they are brought by mere phlegmasia: there is always inflammation, and *something else:* and if what we have so much insisted on be true, this must necessarily be the case, because the state of inflammation succeeds to another, a distinct, and a pre-existing condition of the system: that something else is the unknown, but the peculiar and specific affection of the nervous system, which has already been stated to be the invariable antecedent of whatever subsequent affection may take place. Thus this affection of the nervous system is not only the invariable antecedent of every other condition, but it is omni-present with every other condition, and its presence is a most powerfully influential presence; it operates at every instant, in every organ, and every function of the economy, although, as we have seen, its operation is peculiarly great, and, as far as we can perceive, specific in certain organs and functions. The combination of this nervous affection with inflammation, and the influence which this combination exerts over the inflammatory state, we express by saying that the inflammatory state in fever is modified: we see that inflammation is present, but we see that it is not the same as inflammation in a pure phlegmasia: we see, as has just been stated, that there is inflammation, and something else superadded; namely, a peculiar affection of the nervous system, which gives to the febrile inflammation a peculiar character, or which modifies it in a specific manner."

Having finished his exposition of his theory of fever, the Doctor expresses his satisfaction with his work, in terms of high and unqualified triumph.

"This view of the constitution of fever appears to explain in the most luminous and complete manner every peculiarity of the febrile state: to reconcile all its apparent anomalies, with which few who have studied the subject have not been perplexed: to establish the true distinction between fever and inflammation; and to show why the phenomena exhibited by these two affections are so essentially different, and why therefore each requires a different mode of treatment. In this point of view

no theory was ever more eminently practical, or led to a more guarded practice. Inflammation does not lose its nature by being combined with that peculiar affection of the nervous system which converts it into fever; it only modifies its state: the remedies proper for fever do not differ from those which are effectual in inflammation; they only require to be modified in accordance with the modified nature of the disease. He who believes fever to consist of an affection of the nervous system alone, every other affection that may be combined with it being accidental, will rarely think of using the lancet: he who believes fever to consist of inflammation alone, and overlooks the presence of the nervous affection, will be apt to carry the employment of the lancet too far: he alone who embraces the view of both, brings within his own all the phenomena: he alone adopts a sound theory of the disease, and we now see that he alone is likely to be led to a sound practice. When the theory of a disease collects, arranges and points out the true relation between all its phenomena, there is good reason to conclude that that theory is sound; but when it moreover directly leads to that treatment of the malady which experience shows to be the most safe and the most effectual, its truth is established by every test that can be applied to it."

Such is the encomium which the Doctor passes on his own production. To attain the pinnacle of self-gratulation, and render his exultation complete, he has only to induce the church to perform a Te Deum, in thankfulness for his conquest of that which had previously resisted labour, and set genius at defiance. Had Napoleon subdued the Muscovite, and assumed the title of "Emperor of the West," he could scarcely have announced the achievement in thoughts of greater pomp. His diction might have been more gorgeous, but its meaning must have been the same. Cæsar's *veni*, *vidi*, *vici*, although but three plain words, of two syllables each, express as much of individual pride, and of the loftiness of the conqueror, as any phrase in any language. Time will determine whether the medical world will confirm our author's claim to the chaplet he has assumed. We doubt greatly the "adhesion" to him of either the Italian or the German school, and we shall hold ourselves prepared to hear him charged with usurpation, by that of France. Our own opininon on the subject, has been already expressed in part, and shall be made more fully known hereafter. We shall only add, that, be the issue what it may,

the Doctor is evidently indebted for some of his happiest thoughts to Dr. Armstrong; a fact which he appears to have forgotten to acknowledge. He recapitulates his theory of fever in a single paragraph.

"In conclusion, then, the doctrine of fever which appears to approximate most nearly to the truth, may be summed up in few words. The immediate cause of fever is a poison, which operates primarily and specifically upon the brain and the spinal cord. The diseased state into which these organs are brought by the operation of this poison, deprives them of the power of communicating to the system that supply of stimulus (nervous and sensorial influence) which is requisite to maintain the functions of the economy in the state of health. The organs, the seats of the functions, deprived of their supply of nervous influence, become deranged, the derangement in each taking place in a fixed order, and in a determinate manner. Subsequently to the nervous and the sensorial, the organs the next to suffer are those of the circulation; then those of respiration; and, ultimately, those which belong to secretion and excretion. The condition of the nervous system which produces this derangement in this circle of organs, occasions further, in that portion of the circulating system which consists of the capillary blood-vessels, that peculiar state which constitutes inflammation: hence inflammation is almost always established in one or more of the organs comprehended in the febrile circle, and sometimes in all of them. The peculiar and primary affection of the nervous system, which is here assigned as the cause of inflammation, does not become identical with inflammation, but superadds the morbid condition of inflammation to its own; does not lapse into or terminate in the inflammatory state, but accompanies it, and by this combination modifies in a peculiar manner the inflammatory process."

Chapter VIII treats of the "causes of Fever," and is greatly inferior to either of the preceding ones. It is wanting in almost every thing essential to science, and bespeaks, in the writer, but a very moderate acquaintance with his subject. It is made up almost entirely of the reports and opinions of preceding writers. From this it would seem probable, that the author has had but few opportunities of observing for himself, on a wide scale. To analyze a chapter, consisting thus of mere common-place, would be to throw away time. We cannot however withhold an expression of our astonishment at the Doctor's ignorance of the cause of the cessation of the plague

in Grand Cairo, and of every material circumstance connected with that event, about which so many observers, real or pretended, have written both in former and latter times. That we may run no risk of misrepresenting his meaning, we shall again quote an entire paragraph. But we must first notice his notion of the origin of plague in general. The following is his own exposition of it.

"Without doubt, a febrile poison, purely of animal origin, in a high degree of concentration, would kill instantaneously; and when not intense enough to strike with instantaneous death, it would produce a continued fever with the typhoid characters, in the greatest possible degree of completeness and perfection. And this appears to afford the true solution of the origin of the plague. The more closely the localities are examined of every situation in which the plague prevails, the more abundant the sources of putrefying animal matter will appear, and the more manifest it will become, not only that such matter must be present, but that it must abound."

There can be nothing more unfounded than this view of the subject. The pestilence of the east does not arise necessarily from animal putrefaction. Its origin is the same with that of yellow fever, which is now acknowledged, by every one, to be much more the offspring of vegetable than of animal decomposition. In a particular manner, it is not true, that "the more closely the localities are examined of every situation in which the plague prevails, the more abundant the sources of putrefying animal matter will appear." Plague prevails in many places where no putrefying animal matter can be detected. This is a fact of such universal notoriety, that we are much surprised at Dr. Smith's want of knowledge of it. Let him examine the true history of the rise and prevalence of plague, in any part of the world, and he will find that his view of the subject is erroneous. We apprehend that he has been led to the adoption of it, by nothing better than popular rumour. That the origin of typhus fever is exclusively animal, we are inclined to believe. But we are persuaded that yellow fever and pestilence, which we consider identical, are to be referred chiefly to a vegetable source. But we must hasten to our author's obsolete opinion about the plague of Egypt.

"In assigning the reason why Grand Cairo, in Egypt, is the birth-place and the cradle of the plague, Mead states that this city is crowded with vast numbers of inhabitants, who live not only poorly, but nastily; that the streets are narrow and close; that the city itself is situated in a sandy plain, at the foot of a mountain, which keeps off the winds that might refresh the air; that consequently the heat is rendered extremely stifling; that a great canal passes through the midst of the city, which at the overflowing of the Nile is filled with water; that on the decrease of the river, this canal is gradually dried up, and the people throw into it all manner of filth, carion, offal, and so on; that the stench which arises from this, and the mud together, is intolerably offensive; and that, from this source, the plague constantly springing up every year, preys upon the inhabitants, and is stopped only by the return of the Nile, the overflowing of which washes away this load of filth: that in Ethiopia the swarms of locusts are so prodigious, that they sometimes cause a famine, by devouring the fruits of the earth, and when they die, create a pestilence, by the putrefaction of their bodies; that this putrefaction is greatly increased by the dampness of the climate which, during the sultry heats of July and August, is often excessive; that the effluvia which arise from this immense quantity of putrefying animal substance, combined with so much heat and moisture, continually generate the plague in its intensest form; and that the Egyptians of old were so sensible how much the putrefaction of dead animals contributed towards breeding the plague, that they worshipped the bird Ibis, from the services it did in devouring great numbers of serpents, which they observed injured by their stench when dead, as much as by their bite when alive."

True; the sentiments contained in this quotation are derived from Dr. Mead. But no matter; Dr. Smith, by adopting them, has made them his own; and they are almost from first to last unfounded. In a particular manner, it is not true that the plague is suppressed in Grand Cairo, by the overflowing of the Nile. It is suppressed by the occurrence of such a state of perfect aridity of the soil, canal, city, and surrounding country, that there is not in the entire region, the river and covered reservoirs excepted, a particle of water to aid in the formation of the miasm which produces it. This is matter of history, with which it would have been creditable in our author to have made himself familiar, before venturing to become a writer on the subject.

The plague of Grand Cairo terminates uniformly from the

20th to the 24th or 25th of June. At that period, no rain having fallen for many months (for even a shower in that country is almost as rare as snow in Congo) all Egypt is as dry as any spot in the desert of Arabia. Were we not apprehensive of being accounted figurative, we would have said, as dry as a well burnt brick. It is sufficient however, that, with the exceptions already mentioned, it is perfectly waterless. The Nile is now but just *beginning to rise*, and does not overflow until about the *middle of August*. The cessation of the plague, therefore, which is Dr. Smith's *effect*, occurs nearly *two months* before the superflux of the river, which is his *cause!* Such striking mistakes, in plain matters of fact, from whatever cause they may arise, diminish not a little our confidence in a writer. Nor is the tale about the dead locusts in Ethiopia any better. It is as much a creature of fancy, as the fable of the Phœnix. No authentic record exists of plague having ever originated from that cause. Indeed writings worthy of credit give us but very little information about the prevalence of plague in Ethiopia from any cause. The ravages of the disease in that country are matter of rumour rather than of history.

We must acknowledge however that there is some merit, although nothing new, in our author's suggestions, respecting a mode of determining, by experiment, the practicability of transporting the miasm of plague and other fevers, from one country to another, and there propagating the complaints by it. That the result of such a trial might be highly useful, cannot be doubted. Under this belief, experiments of the kind, to settle the question of the contagious nature of yellow fever, were recommended in Philadelphia more than twenty years ago. Dr. Smith announces his intention of discussing more fully, "in a future publication," both this subject, and several other points of importance, connected with the production and general philosophy of febrile miasms. We sincerely wish him success, and suggest to him the probability of his being much more likely to attain it, by relying more on his own observations, and other personal resources, and less on the authority of names, than he has done in this chapter.

In chapter IX, Dr. Smith unfolds his plan of the "Treatment of Fever," which he had previously alleged to be the "most safe and the most effectual" hitherto pursued. Its safety and efficacy in curing the fevers of England, we shall not question. We know that the febrile diseases of different and remote countries require different modes of practice; and we have no experience in the treatment of those of any part of Europe. But the Doctor's plan would be altogether unsuccessful in treating the fevers of the United States. In common with other schemes of European practice, it is, in several essential points, by far too feeble for them.

Notwithstanding what he had previously said, about the difference between fever and inflammation, he now acknowledges, that the "only morbid condition of fever, of which we have any knowledge, and over which the medical art has any control is that of inflammation." Nor, in his opinion, do "the remedies proper for febrile inflammation differ from those which are adapted to ordinary inflammation," except as respects "the mode in which they ought to be applied, and the extent to which they ought to be carried." And what practitioner does not know that, in the treatment of what is here denominated "ordinary inflammation," the same remedies must be applied in different "modes" and in different degrees of "extent" in different cases! In a practical point of view, the question of the essential difference previously alleged to exist between fever and inflammation, is here virtually surrendered.

After fully as much preliminary matter as circumstances call for, our author announces bloodletting as his first and principal remedy. In the employment of this he is courageous; and his general views respecting it are correct. He practises it, very properly, as well to prevent inflammation, excitement having commenced, as to subdue it after it has occurred. And his object, very properly again, is to *subdue*, not merely to *weaken* the inflammatory affection. The war is one of final overthrow. The foe must be exterminated, else he will himself exterminate, or entail on his victim a state of suffering worse

than death. Infinite mischief is done daily, by what is called "safe" or "cautious" practice; by "not carrying bloodletting too far."—No, truly; it ought not to be carried too far; nor ought any thing else. But it should be carried *far enough;* and that means, until inflammation is completely extinguished. We mean, as far as it can be extinguished by this remedy.

But the Doctor relies too exclusively on bloodletting; at least for the fevers of the United States. There is often a point, beyond which it cannot be carried with either advantage or safety; while still inflammation is unsubdued. Other means must then be resorted to; and they are chiefly local bloodletting, secretion, and revulsion. Of secretory remedies, the most powerful and efficacious are those which purge and nauseate or puke. These, indeed, our author recommends; but his doses are so diminutive (we mean his purgatives) and the intervals of their repetition so remote from each other, that they would be unavailing, if not worse, in American practice. Without copious purging the fevers of our country, especially of the south and west divisions of it, cannot be cured; and that can never be induced, by "one or two grains of calomel, with six or eight of rhubarb, repeated every night, or every other night, and followed the next morning by two drachms, or half an ounce of castor oil, or by the common senna draught." Such practice would be as impotent, in a contest with our great fevers, whether of spring, summer, autumn, or winter, as the Lilliputian arrows were, when directed against Gulliver. Efficient American practice is understood only by American physicians, and can be taught, of course, by no body else. It is as pernicious as it is servile in us, to continue to look abroad, for that knowledge which awaits us only at home. Professional independence and individual manhood unite with reason and experience in urging the discontinuance of the practice.

With one of our author's remedies we are peculiarly pleased; and in form and name it is new to us. We do not know that it is recommended by any other writer, or spoken of by the profession as a means of cure. It is his "cold dash," and consists in a continued stream of cold water, poured on the head of

the sick, from a height of from six to ten feet.* It is best suited to the cerebral form of fever. A correct knowledge of it will be most readily derived from the Doctor's own account of it.

"This remedy is known by the name of the cold dash. It consists of pouring a column of cold water upon the head in a continued stream from a height of from six to ten feet. The mode of applying it is as follows. The patient is seated in a large tub; a table is placed at the side of the tub upon which a man stands, and at as great an elevation as his arms can reach, pours upon the naked head of the patient a steady but continued stream of cold or iced water, from a watering-pot without the rose. The stream is made to fall as nearly as possible upon one and the same spot. At first the elevation must be slight, for the shock is too violent if the stream be poured at once from the highest point."

The effects of this expedient are thus described.

"Employed as a remedy, there is no degree of burning heat which the animal economy is capable of producing, no intensity of vascular action, and no violence of pain that can resist its continued application. Sooner or later, usually in from ten to twenty minutes, the heat, though most intense, disappears, the skin becomes cold, the face pallid, the features shrunk, while the pulse is reduced to a mere thread, and the pain of the head, however violent and intolerable, entirely ceases.* After the patient has been wiped dry, which he should be as rapidly as possible, and placed in bed, the symptoms may soon return in all their violence: the same process will again remove them, and as often as the former recur the latter must be repeated. Three or four repetitions will commonly suffice to subdue the most intense cerebral affection."

The treatment of Dr. Dill, the friend and colleague of Dr. Smith, by this remedy, forms a case of peculiar interest, is highly instructive, and worthy of the attention and remembrance of the reader. It is detailed in pp. 414 and 415 of the original work. Although, as already mentioned, we have never seen the "cold dash" formally practised, we know enough of the character of cold water, and its action on the human body, especially when falling on it in a heavy and never changing stream, to be convinced that, thus employed, its effects are

* The process of cold "*douching*," recommended by other writers, although similar in kind, is so inferior in degree, that it can scarcely be called identical.

powerful. When judiciously directed, therefore, we doubt not its being a remedy of great value.

This notice of Dr. Smith's "Treatise on Fever" is submitted to the reader, as a mere antepast of what he will receive from the work itself. The volume is therefore recommended to his attention, as a production of real merit, though marked by many blemishes. We shall conclude with a few more general remarks.

In this age of intellectual enterprise and general improvement, medicine receives its full share of attention and development. The exertions that have been made, in its cultivation and advancement, within the present century, have been ardent and persevering, wisely directed, and therefore successful in their result, far beyond those of any equal period in antecedent times. It is not extravagant to say, that, in the lapsed thirty years of the nineteenth century, the profession has advanced much more, on the basis of fact and established truth, than it had done during the whole of the eighteenth. Yet, within the last named period flourished many of the most highly gifted and learned physicians, that the world has contained. Stahl, Boerhaave, Hoffman and Haller, the Monroes, Cullen and the Hunters, and several physicians that adorned our own country, formed a professional constellation unsurpassed in brilliancy. Yet it would be folly to deny, in mere deference to those great men, that, at the present day, physicians of ordinary talents, aided by the common lights which surround them, possess more of true medical science than either of them did. They possess we mean, a more correct and accurate knowledge of the animal fabric and economy, in health and disease, and are better skilled in the art of converting the latter into the former. To what is this owing? The answer is plain. Authors no longer write, nor teachers teach, according to systems formed in the closet; nor do pupils study through means so unsubstantial. Things relating to the profession are now taught, and listened to, and looked at, as they really are, in the chamber of sickness and the dissecting rooms; and the latter places, in particular, supply the knowledge, which was

wanting in the former. A large proportion of the light in medicine, which now exists, has been derived from post mortem examinations; in other words, from the cultivation of pathology; and that again is studied only through physiology and anatomy. Mere theory has been abandoned, and all things are becoming more and more positive and practical. Sober reality has taken the place of fiction. Fresh nature is studied and relied on more, and musty books of authority less. And when such books are consulted, it is for facts, not fancies.

By this plain, practical, common sense course of proceeding, the profession is nearly freed from the thraldom of two long-prevailing and pernicious errors; a belief in humoral pathology, and in the existence of ontological or idiopathic fever; we mean fever without any topical affection. The prevalence of these two false notions was the source of many others, which led to fatal results in practice.

Another point. To what cause is this new and improved mode of cultivating Medicine, with its manifold advantages, to be attributed? We answer, to the vast and exciting scenes of difficulty and danger, which have presented themselves to the world within the last forty years; we might say more accurately, within the last half century, commencing with our own revolutionary struggle. Ever since that memorable and awakening epoch, men have looked at things in general as they really are, much more than they did before. But actual improvements *in medicine* are to be referred chiefly, as respects their beginning, to the prevalence of yellow fever in our own country, the West India islands, Spanish America, and some parts of Europe. By making them contend for their own lives as well as the lives of their patients, with a new foe of unusual power, that disease compelled physicians to provide themselves with new means of warfare. Hence began the practice of autopsic researches, in a degree far beyond what had previously existed. The French revolution soon followed, and elicited from the mind of Europe all the light that collision amidst scenes of grandeur can produce. If great captains were necessary to lead armies to victory and

triumph, great physicians and surgeons were no less so, to heal their diseases, and cure their wounds. And wherever human greatness of any kind is wanted, it will certainly be found. The emergency forms it. The five and twenty years of war in Europe, in which the United States ultimately took part, evolved a much greater amount of professional light, than an entire century of peace could have done. Such, in conformity to the laws of nature, are the countervailing advantages, which calamity never fails to produce. The genius of the MAN who directed the affairs of Europe, and left his mark on every thing he touched, was eminently practical. He drove speculation from his favour and his presence. This induced those who served him to become practical also, partly from imitation, and in part from interest, and compelled his enemies to become so, in their own defence. The physician was obliged to report on the nature of the sickness of the soldier; and a mistaken report might disgrace and ruin him. A correct knowledge of pathology therefore was anxiously sought after, and rapidly attained. Owing to this spirit and state of things, we can scarcely doubt, that, with a view to ascertain the seat and character of febrile diseases, a greater number of dead bodies have been opened and inspected, within the past period of the present century, than had been examined before, for the same purpose, since the birth of medicine. The beneficial result of this is seen and enjoyed, in the luminous and practical views, which the profession now possess, contrasted with the obscure and visionary ones, which prevailed only half a century ago. And the light is still increasing.

We have intimated that it is the seat and pathology of *fever*, which have constituted the chief objects of research with the Faculty. It is right that it should be so. Fever, in its various forms, is a composite disease, made up of many others; we might almost say, of fragments of all others. It constitutes moreover the widest outlet of human life. It is computed that one half of our race die of it. To understand fever thoroughly, therefore, in its pathology and treatment, is to be skilled in medicine.

By his lectures and publications on this subject, Broussais has not only taken the lead of all his enlightened countrymen, but has formed a new school. His doctrines are called "*physiological medicine*;" for what good and exclusive reason, we do not exactly know; nor shall we stop to inquire. All diseases involve deranged physiology; and all practice must be founded on physiological principles, else it is empiricism. Every view of fever, therefore, is physiological, as far as it is correct; and it is not yet certain, either that the Pathologist of France is right, in every point of his doctrine, or that others, who differ from him, are always wrong.

That able and intrepid teacher represents fever as a disease essentially abdominal, in which we think him right; but he derives it exclusively from an inflammatory affection of some portion of the mucous lining of the alimentary canal, chiefly of the stomach, in which we apprehend he is wrong. We are inclined to believe that local irritation and derangement, *short* of actual inflammation, produces fever; and we are confident they often do, when carried *beyond* that point. Inflammation necessarily implies reaction. But, in the very worst cases of fever, there is no reaction, local or general. The original impression made by the poison is too powerful, and the consequent *devitalization* of the system (might we form a word to express our meaning) too complete, to admit of reaction. The patient sinks into torpor, and his functions yield without a struggle. Such is the case in every congestive form of fever. The same is also true of the worst attacks of cholera morbus. Under that modification of the latter complaint, called "*mort de chien*," which kills in a few hours, no more reaction occurs, than does in the system of the criminal, whose thorax is crushed by the foot of the elephant. Nor does any more appear in congestive cases of plague, yellow fever, or scarlatina, which extinguish life almost as suddenly. But, in most forms of fever, inflammation does exist in some part of the mucous lining of the primæ viæ. In the main, therefore, Broussais's views are correct. They are especially so, as relates to the belief, that the febrile miasm makes its first attack on the internal sur-

face of the stomach, and that all that follows is symptomatic of the læsion there produced. But that læsion is not inflammatory from its commencement. Nor is any other. It only becomes so, in certain cases, by organic reaction.

In another respect the doctrines of Broussais are calculated to do much good. They withdraw the attention of the profession from the large blood-vessels, as the seat of fever, and direct it to the capillaries and the parenchyma, where it ought to rest; for the complaint is certainly radicated there. All morbid appearances in the large vessels are but symptoms. Those vessels are inaccessible to the miasm of fever, until it shall have first passed through the tissues composed of minute vessels, and injured them; for we see not how it can penetrate them, without doing them an injury. We are persuaded that it does not penetrate them at all. An opinion, the contrary of this, appears to us to be unsupported by either fact or analogy. We are ignorant of any thing reasonable that can be said in its defence. It cannot be made to appear even specious. Certainly to our vision it has never assumed an appearance so respectable. Broussais' view of fever then is very simple. By some malign impression inflammation is excited in some portion of the mucous lining of the alimentary canal. This is the primordium or root of the disease; and all that follows is the work of sympathy. Let the inflammation be removed, and the complaint is cured. According to this doctrine fever is always and necessarily symptomatic.

It has already appeared that Dr. Smith, the author of the work we have been examining, is opposed to this theory. Fever, he says, begins in the nervous system, and its groundwork is very different from inflammation. Inflammation is but something occasionally superadded to it. It is but an accidental accompaniment, not an essential element of it. In the lightest and the worst forms of fever there is no inflammation. That which is called symptomatic fever, he asserts is not fever at all, but inflammation. It belongs to Cullen's "Phlegmasiæ," not to his "Febres." Fever, moreover, he says, when fully formed, has its stronghold or seat, not alone in the stomach, or

any or all of the chylopoetic viscera. It occupies also the viscera of the thorax, and the brain and spinal cord. All this, he contends, is abundantly proved by symptoms during life, and dissection after death.

We have already assigned our reasons for dissenting from Broussais, in some points of his theory, especially his position, that actual inflammation is essential to fever. We are deterred, by reasons equally strong, from concurring in all respects with Dr. Smith. Unless dissections as well as symptoms are fallacious, the fevers of the United States are not seated alike in the viscera of the three great cavities. We allude to the fevers of summer and autumn. They are much confined to the viscera of the abdomen. True; the lungs and heart, with the brain and spinal cord, suffer. But they suffer sympathetically, and in a subordinate degree. Primarily and principally they are never affected; or if principally, it is only in abnormal cases. We have seen many dissections in yellow fever, in which the lungs and brain were free from the slightest appearance of disease. In some of these cases, moreover, delirium had existed. But it was sympathetic. Inflammation and congestion were confined exclusively to the chylopoetic organs. In our common autumnal fevers the same is true. Their seat is below the diaphragm. Indeed that muscle is a kind of partition between the seats of febrile diseases in cold and hot climates. In the latter regions, the organs beneath it suffer most; in the former, those above it.

Our dissent from Dr. Smith's opinion that there is an essential difference between fever and inflammation, we have already expressed, and shall not pursue the subject further. But our strongest objection to the Doctor's theory relates to the origin of fever. The læsion producing it is not first made on the cerebral system. That we conceive impossible. It must be made, of necessity, on some of the outposts of the body; some part accessible to miasm. It cannot be produced in any other place. But the only parts which the poison can reach are the skin, the mucous lining of the primæ viæ, and that which lines the nares, trachea, and bronchiæ. One or another

of these surfaces, therefore, or the whole of them, must receive the first impression of the miasm. And, as already intimated, that impression is not made on the nerves alone of the organs acted on. It is made on the whole of the tissue exposed; but the nerves of the part are chiefly concerned in conveying it to the distant portions of the body. That the poison itself is conveyed by the blood from the lungs, or by the absorbents from the skin or the alimentary canal, cannot be maintained. No evidence to that effect of the least weight has ever been adduced. Nor does any such exist. All that has ever been advanced on the subject, in the form of evidence, is but loose analogy, or mere conjecture. The hypothesis has at present, but few advocates of any distinction.

Broussais' theory of fever has the advantage of Dr. Smith's in being more simple and intelligible. It has less of ontology and mystification in it. It does not suppose the existence of any thing of great moment, which it is unable to express. It considers inflammation, with fever or without it, the same affection, differing only in degree, and exhibiting different phenomena according to the tissue and the organ inflamed. Dr. Smith has not made any thing to the contrary of this appear. He represents inflammation as only accessory to something else, which is the ground work of fever. But he does not tell us what that something is. We however tell him, that in every instance, where the original impression productive of fever is of a certain grade, inflammation occurs, and gives both character and continuance to the complaint. And the inflammation differs from common inflammation only in the particulars already stated. But if the impression be below or above the grade referred to, inflammation does not follow; certainly it does not follow in the latter case. The fever is then congestive. But the same is true of irritation from other causes, as well as from febrile miasm. A burn of a particular degree of violence produces inflammation and fever. When still more violent, it kills in a few hours, by what is strictly congestive fever. Of gunshot wounds the same may be affirmed. When very severe, they kill without a fever of reaction. Gene-

ral Moreau had his lower extremities cut off, above the knee, by a cannon bullet. But he did not die of hemorrhagy. That was prevented. Nor did inflammation ensue. The impression made was beyond the inflammatory grade, and the hero died of what may be correctly called a congestive fever; a general want of reaction. The overwhelming shock given to the nervous and cerebral system, prostrated all the vital and recuperative energies, and prevented reaction. A moderate dose of arsenic produces gastric inflammation and fever. A very large dose throws the system into a congestive condition, and in that way kills.

It was our intention to have said something of the theories and treatment of fever, maintained in the Italian and the German schools, especially the counter-irritancy of Rasori, and the homoopathy of Hahnemann. But having already trespassed on the usual limits of a review, and we fear also on the patience of our readers, we forbear for the present. A single remark further shall close this article. When we seriously consider the striking difference, not to call it positive opposition, which marks the different theories of fever and the modes of treating it, and again consider the mass of testimony equally voluminous, and apparently alike respectable in favour of each, we are puzzled not a little to decide, how far it is safe to confide in the correctness of human perception, or the soundness of human veracity. We are even almost tempted to resolve on believing nothing until we have seen it.

C.

MISCELLANEOUS INTELLIGENCE.

[The cholera still occupies much of the attention of Europe, and has made its appearance in England during the past summer. The last number of this Journal, in consequence of the great importance of the subject, contained such intelligence as we could readily obtain respecting it—to which were added some remarks which through inadvertence were not stated to be by one of the editors. (See p. 425.) The following letter upon the same subject will, it is thought, be deemed very interesting; and the reader will find that the facts stated in it, strongly confirm the views given in the remarks above alluded to. The letter is taken from *the Albion*.] C.

Dr. Brown, of Sunderland, to Dr. James Johnson, and Dr. Alexander Tweedie, Physician to the London Fever Hospital.

Gentlemen—The first question asked here, and which probably may be put in London, is, "Is this the continental disease?" The question is an ambiguous one, and ought not to receive a direct answer; for it may mean any one of these three things, viz.—1st. Do its symptoms correspond with those of the continental disease? 2d. Has it been imported from the Continent? 3d. Does it, in its diffusion, obey the same laws as the continental disease? In the first sense of the question, I would answer it in the affirmative; in the second, *decidedly in the negative*; whilst in the third, my answer would be doubtful, for the laws which the continental disease obeys, in its diffusion, are not as yet fully ascertained.

Need I examine formally the question of its importation, and refute the story circulated through the newspapers of certain ships which lay above our bridge, and communicated the disease to the town? Those ships came from places where cholera did not exist at the time of their departure—most of them from Holland, where it has not yet appeared; their crews were, and had been, in perfect health; and the disease *first* manifested itself in a part of the town, *two miles distant from where they were lying*. If there have been other modes in which disease may have been communicated from the continent, I know not of them. One man, a pilot, was attacked some time ago;

he had been on board a vessel, which had recently come in ballast from performing quarantine; but at the same time, cases identical in character were taking place in individuals who had had no communication with shipping; and assuredly a ship with a healthy crew, in ballast, and which had performed the regulated quarantine, was not a probable source of the disease with which this man was attacked, and under which he succumbed.

The importation doctrine is here—where we must be supposed to be the most competent judges of a matter not of opinion but of fact—so generally abandoned, that I shall bestow no more pains on its refutation; but proceed to give as concise a sketch as I am able, of what we may designate by the antiquated term, the medical constitution of the latter part of the present year, and endeavour to show by what insensible gradations, our autumnal disease has passed into the intense form which has produced such lamentable consternation throughout the empire.

Early in the month of *August*, cholera appeared and speedily became very prevalent. It ranged in all degrees of intensity, from slight bilious attacks, to cases attended with violent spasm, coldness, collapse, almost (if not complete) arrest of the circulation, *white discharges, suppression of urine, and, in short, all the symptoms ascribed by observers to the Asiatic and Continental diseases.* Of these more intense cases, several were fatal, some of them within *twelve hours;* whilst others narrowly escaped by prompt and skilful medical assistance. Such cases occurred in situations remote from each other; some of them several miles *inland*—one, for instance, and that a fatal one, in a female living in the village of Boldon, five miles in the interior, and remote from the river. Others of the agricultural population suffered in various situations: some certainly near the river, but there were no ships in at the time, which had come from suspected places.

On the abatement of the heat, cholera became less general; but did not totally cease, cases continuing to recur at intervals, some fatal, others of great intensity, but terminating favourably; whilst the prevailing gastric and intestinal constitution was marked by the frequent occurrence of cases of fever, commencing with vomiting and purging of matters variously coloured—in short, by symptoms of cholera; and this state of things continued till the almost stimultaneous occurrence of four deaths from cholera, on the 31st ult. and 1st inst. excited general alarm. For what has subsequently occurred, the reports to the Board of Health must be referred to.

Whilst matters are thus proceeding in Sunderland and its immediate vicinity, information I have received from various channels, leaves no doubt on my mind, that a similar train of events has been in progress generally throughout the north-eastern division of the kingdom—the same prevalence of fever, of which the initiatory stage is marked by vomiting and purging—the same occurrence of fatal cases of cholera, since the season of heat and fruit had passed; but, so far as I know, the same prevalence of the intense forms of the disease has not been manifested elsewhere as here; yet this difference in degree does not, I imagine, make our state *essentially* different. What is our fate to-day may be that of others to-morrow. A fortnight ago we were no worse than our neighbours.

In this sketch of the general progress of disease, during the autumn, I have classed together things which, because differing in name, are by many supposed to be *essentially* different—cholera and fever. The relation between these two genera of disease, is as close as possible. If a man is attacked with vomiting, purging, and collapse, and succumbs to these symptoms, he is said to die of cholera; if the disorder of the system is eliminated, as it were, by the discharges from the intestinal canal, he is said to recover from it; but if neither of these events occur, his state becomes one of fever, not distinguishable from that affection unpreceded by choleric symptoms. The first case which Dr. Daun saw, on his arrival here, he said he could not distinguish from typhus,—the discharges had ceased, and it had passed into the febrile state. I pointed out this relation between ordinary cholera and fever in the Essays published three years ago, and was not a little surprised to see my observation quoted in the *Courier* newspaper of the 7th inst. Am I then to regard the disorder I now witness here, as something not resulting from indigenous causes—as in no way connected with the progression of disease I have remarked —but as suddenly thrown in and totally foreign from that to which, notwithstanding, it bears so striking a resemblance? I own I cannot so far shut my eyes to facts, or fail to exert my reason in drawing legitimate inferences from them. If I now visit the houses of the opulent and comfortable, I find, as heretofore, bilious attacks easily allayed, or febrile disorder connected with them;—in the hovels of the indigent, I find a disease characterized by the symptoms of the epidemic cholera of the Continent, so far as I can judge of them by the reports of others. The rational inference from this appears to be,

that both result from the same epidemic influence, modified in its operation by the different circumstances in which the two classes of persons are placed.

In thus stating my belief, that this disease is but the general epidemic influence, I wish it to be distinctly understood, that I have seen nothing in its progress that has led me to conclude that contagion is instrumental in its diffusion. One or two *prima facie* cases of infection; but a little scrutiny has shown me their fallacy. This branch of my subject, if it appear necessary, I will resume at a future opportunity.

It must be evident from the opinion just expressed, and the general tenor of my communication, that I should deem all restrictive measures on internal commerce as useless in a preventive point of view, as they have already proved on the continent, whilst, on many grounds, their impolicy is obvious. I remain, my dear Sirs, Your very obedient servant,

J. BROWN, M. D.

Sunderland, Nov. 10, 1831.

Medical effects of Oxygen.—A respectable physician of London has given us two cases of the administration of oxygen gas as a remedial agent, which deserve the attention of the faculty. One of these evinces the power of this gas over the organs of voice, and both tend to confirm the opinion of its general influence as a tonic. Mrs. S. who was the subject of the first, had long suffered from an entire loss of voice, a complaint not at all uncommon, and which frequently baffles great skill in treatment. She was of nervous temperament, and had been many years subject to distressing spasmodic affections, requiring the frequent use of opium in large doses. In about a month after commencing the use of oxygen, diluted with three parts of atmospheric air, the general health began to improve. Her nervous attacks became more rare, her countenance and strength improved, the voice began to be heard, and at the expiration of five months, its tone became natural, and its power as great as before her illness.

The other case was one of great debility from protracted disease of the eyes, and the confinement, despondency, &c. consequent on its unsuccessful treatment. After a few weeks inhalation of the diluted oxygen, her sleep returned, her spirits rose, her countenance and health improved, her pulse became less feeble, and the blood taken from the vicinity of the eyes, from an almost watery transparency, became of its usual florid colour.

The invigorating effect of oxygen on the system has been fully confirmed by past experience, but its special power of giving strength to the organs of the voice is not so generally known. The celebrated Mrs. Siddons was in the habit of availing herself of this agent when preparing to charm a London audience. After inhaling six quarts of oxygen mixed with twice the quantity of atmospheric air, her lower tones were distinctly heard at the distant parts of that immense house, the Covent Garden Theatre; and her strongest efforts were not followed by that exhaustion, sometimes amounting to syncope, which at other times followed them. Mr. Melrose is said to have adopted the same expedient, and to have been able, by the agency of this gas, to raise his voice one or two notes higher than he could do without it. Other instances are on record, in which the same agent has been successfully employed by vocalists, to increase the scope, and give additional vigor to the voice. It is said, also, that clergymen in England have tried this experiment, and the happy effects of it have been noticed by their hearers.

We are not aware of any case in which oxygen has been used in this country in the cure of disease. Should suitable cases offer to any of the faculty, we trust they will not be behind their transatlantic brethren in bringing to the aid of their patients this valuable discovery of modern science.—*Boston Med. and Surg. Jour. No.* 13, *vol. V.*

Use of cotton in dressing wounds. By Dr. Peschier.—In a letter addressed to the Editors of the Bibliotheque Universelle, Dr. P. states that he has proved with entire satisfaction to himself, that the general opinion of the unfitness of cotton for the purpose of dressing wounds, is altogether an unfounded prejudice; and that carded cotton, employed either as lint or bandages, is in fact preferable to linen. He does not pretend to be the discoverer of this fact, but refers it to an incidental circumstance which occurred in America. A child which had been most severely burned, was laid upon a heap of carded cotton, while the person who first rescued the child went for assistance. On returning, instead of finding it in agony, it was fast asleep; and the wounds, though deep, healed rapidly, with no other application than the soft cotton, which they did not venture to detach. It is, Dr. P. remarks, in the most desperate cases that cotton is the most useful in burns. When the skin, and even the flesh, has been shrivelled and roasted with the heat, the application of cotton has been found to promote

the sloughing and suppuration without too much pain, thus preserving the life of the patient, otherwise so doubtful under circumstances of this nature.

Dr. Peschier cites the following cases, which came under his own notice. Two artillery men, in charging a cannon too hastily, had their hands and faces so severely burned by a sudden deflagration of the cartridge, that the epidermis was separated. The injured parts were immediately covered with cotton, and so successful was the application, that although the hands, and especially the face, were prodigiously swelled, the eyelids tumefied and the nostrils obstructed, the wounds healed so perfectly that not a trace was left of the accident. So much for burns.

A family of five persons were attacked with typhus, which reduced them to great extremity. They recovered, but one of them, a lad of twelve years of age, became a prey to enormous eschars, on all the parts of his body which were obliged to sustain any pressure, or even a permanent contact: that on the sacrum was at least six inches in diameter, those of the trochanters were the one five and the other four inches, and on each knee was one of two inches, with smaller ones on the feet. The young man was reduced to the lowest degree of emaciation, and so great was the pain that his cries were almost incessant day and night for nearly a month. After trying in vain the ordinary means, Dr. P. thought of carded cotton, and of this he applied a thick compress on each wound. On the first night after, the patient slept, the pains abating as if by enchantment. The application was continued, and in the month of February, the eschars, which had commenced in September preceding, were reduced to very small simple wounds, and the patient had regained his strength, notwithstanding the enormous suppuration.

The precaution which was observed in this important case, and which Dr. P. considers indispensable to success, was never to change the compress of cotton, except when the amount of suppuration incommoded the patient and almost entirely detached the mass. In dressing, also, great care must be taken to cut with good scissors, and never to pull out the fibres of cotton which adhere to the borders of the wound.

Such unexpected success induced the belief that every kind of wound or ulcer might be treated with decided advantage by dry carded cotton. An opportunity of trying it was soon presented. An

unfortunate being, with an enormous cancer of the face, was dressed with cotton, without experiencing from it the least pain. The disease being in its nature incurable, Dr. P. did not pretend to heal it by these means, but he rendered the treatment of it far more supportable.

All kinds of wounds, simple and complicated, have been thus kindly and promptly healed. A wound of the head, complicated with much hemorrhage, was by the same means successfully treated. A prejudice exists that cotton is dangerous to the eyes, but Dr. P. states that he had speedily cured a man who, by falling on some stones, had received a severe contusion of the face, a corner of one of the stones having penetrated and torn the flesh.

Obstinate scrofulous ulcers have been treated in the same manner, with no less success.

"I cite these facts (observes Dr. Peschier,) only to demonstrate that cotton is applicable, indiscriminately, to all kinds of wounds and ulcers, and that far from being, as it is unjustly regarded, *poisonous*, that is to say, *irritating*, it furnishes, on the contrary, a material for dressing wounds, of the softest and most pleasant kind. But, I repeat, it is indispensable to success, that the dressings be rare, and that the threads be never pulled or torn from the wound,—a practice which cannot fail to increase either its extent or aggravation. The scissors, lightly handled, must be used to separate from the adhering fibres, the mass, which may be safely detached.

"I would be the first to admit that there is very little scientific merit in substituting cotton for lint; but I deem it to be rendering an important service to the wounded, to their connections, and especially to the attendants at civil and military hospitals, to convince them that they need not be uneasy at the difficulty of procuring lint, a substance not always easy to preserve,—which becomes easily infected with miasmata, and which cannot be kept in large masses without some danger. Carded cotton is found everywhere; it is of trifling value, so that the rich will at no time refuse to buy it for the poor, and hospitals can be at all times well provided with it.

"The same remarks apply to cotton cloth. It is of trifling cost, even when of the finest kind; it has precisely the degree of suppleness which fits it for bandages and compresses; it occupies vastly less space in travelling chests than linen or hemp, and it may be any where abundantly obtained."—*Ibid. No.* 15, *vol. V.*

*New mode of giving Calomel in the treatment of Syphilis.**—A butcher, 28 years of age, contracted syphilis in the course of the last year; and applying for relief to certain practitioners, who disgrace humanity by their vile charlatanism, obtained from them a large quantity of corrosive sublimate, disguised by the admixture of other drugs. The sores got well, or disappeared rather, under this treatment. But, some months after, the man suffered extremely from headach of the most excruciating intensity, of which emetics and bleeding procured him some alleviation. An eruption on the face now made its appearance, chiefly affecting the eyelids and the nose, and this brought him into the hopital St. Louis. Here, as he showed symptoms of *gastro-enterite*, he was kept on slops, and such a regulated diet as seemed to check the progress of his disorder for a considerable time: all of a sudden, however, tubercles covered a great portion of his skin. He was ordered the *aqueous extract of opium*, with no success; then other methods, without amelioration. M. Biett now thought he should try a new mode of treatment, which suggested itself to him. This was to give the patient day after day an errhine, composed of a certain quantity of calomel, combined with an inert powder. In this way the man took successively, *eight, twelve, fifteen, twenty grains*, daily; and before a month was well elapsed, found his tubercles gone, his ulcerations cicatrized, and his general health so much improved that he was able to leave the hospital. He came in again subsequently for an inflammation of the synovial capsule of the knee, which was got down by cupping, leeching, and emollient applications. Some new pustules of his old complaint being observed, he was put on calomel once more, given in the manner just mentioned; and the consequence has been so satisfactory, that there seems to be no likelihood whatever of a return of the disorder.

M. Biett has employed calomel in powder in several other cases of secondary syphilis, and always with the most satisfactory results. Besides giving it in the form of an errhine, he applies it externally sometimes to venereal ulcers, and his success is truly remarkable.—*Ibid. No.* 9, *vol. V.*

Case of Colica Constipata removed by inflation. By John King, Jun. *Surgeon.*†—The importance of inflation as a remedy for obstruction of the bowels, appears to me not to be sufficiently appreciated

* Gazette des Hopitaux. † Glasgow Med. Jour.

at the present day. It was first recommended by Hippocrates for the removal of intestinal obstruction; in more modern times, it has been resorted to by Hoffman and Haller; and notwithstanding the neglect it has since experienced, I cannot but regard it as worthy of an eminent position in the list of therapeutic agents. The treatment usually prescribed in cases of ileus or colica (without inflammation) is very discordant, as witness,—warm baths, fomentations, injections of warm water and oil, rubefacients, and blisters,—contra—cold affusion and immersion, freezing lotions, pounded ice and snow; not to mention emetics, purgatives, and mechanical distention by warm fluids, quicksilver, gold and silver balls &c.—and when all these remedies have failed, bloodletting, tobacco, in infusion and smoke, and lastly, gastrotomy. Yet this simple means of inflation, although probably the most powerful, and the least dangerous, is entirely overlooked. It paralyses, as it were, the constricted fibres of the bowels, and may be used in the following cases, if not with complete success, at least with advantage, viz. the various kinds of colic proceeding from torpidity, spasmodic constriction, viscid meconium in newborn infants, impaction, bezoards, and other intestinal concretions, volvulus or intus-susceptio, and some cases of hernia. It was a happy thought of those who hit upon this means in the hour of danger, after all their other efforts had proved nugatory. For although tobacco, which is often used as a last resort, sometimes is successful, it is not uniformly so, and it too often happens that the patient, rather than undergo a repetition of it, beseeches to be allowed "to die in peace." We may also observe the hesitation with which the practitioner has recourse to it not only because of its doubtful efficacy, but on account of the danger there is of greater exhaustion being produced by it. I take the liberty of giving one case, as I conceive it may give some idea of the power of inflation.

In September, 1829, I was requested to visit Mrs. G. æt. 26, of rather delicate frame. On the night previous to my visit, she experienced an uneasy sensation in the region of the stomach, for which she took eight grains of calomel combined with a half-drachm of compound powder of jalap, without any impression on the bowels. During the night, this uneasiness increased to an almost intolerable pain, accompanied with obstinate vomiting, which continued till the evening, when I saw her. In the course of the day she took two doses of castor oil and received five injections. When I entered the apart-

ment, she was sitting near the fire, and her body bent forward; the face was wan, hollow, dejected, and of a dingy yellow color; the surface of the body and extremities inclining to cold. Pulse 80, soft, and much compressed—tongue, at the back part, covered with a brownish-coloured mucus—she had obtained no alvine solution for six days. She took no notice of my being present, or of any thing going on around her, but informed me, when questioned as to the seat and kind of pain, that it was of "a violent screwing nature, working between the stomach and navel,"—coming on in paroxysms, and ending in, or producing vomiting. I ordered the warm bath, and gave a teaspoonful of laudanum with compound spirit of lavender, which was soon afterwards vomited. Upon this, an effervescing mixture was given, then five drops of croton oil with some laudanum, and in about three quarters of an hour, five drops more without laudanum: but each in its turn was rejected, with a quantity of yellow-colored fluid. It was at this time I first thought of inflation. For this purpose I procured a pair of common bellows, and securing the bladder of a glyster bag to the nozzle of the bellows, the pipe was introduced into the rectum, while the patient lay on her right side, and the bellows was commenced being wrought. As soon as the air entered the rectum, the effect was immediate and satisfactory; the countenance lost its anxiety, the eye brightened, and the patient said she felt quite relieved. A gurgling noise was heard in the bowels, with an escape of fetid air; and in about a minute from the time the air began to enter the rectum, she requested to be allowed to go to stool. She had a copious dejection and a good night's rest; and next morning complained only of being much enfeebled, but was otherwise well.

I was deeply impressed, about five years ago, with the fatal result of a case of intus-susceptio, in a fine robust infant, six months old; which was supposed to proceed from the effect of half a teaspoonful of some syrup of poppy, made, as is commonly done, with opium, given for the purpose of procuring sleep during the period of teething. About eight hours after it was given, the child began to cry vehemently, having appeared restless and uneasy for several hours previously. Early in the forenoon it passed a very scanty stool, streaked with blood; soon after this, vomiting commenced, which continued until the little sufferer sank. Is it unreasonable to imagine that if inflation had been used in this case, the result would have been otherwise? I was hereby shown the necessity of seeking more powerful

means than fluid injections, &c. And I hope, as I firmly believe, that inflation with common air is the necessary desideratum. I conclude, with Dr. Cheyne, "that a man dying of ileus presents one of the most pitiable sights in nature; and a leading object of this paper is to remove a part of the horrors of the scene, by withholding many of the bitter doses which are forced upon him by the solicitude of his friends and the officiousness of his physician."—*Ibid. No.* 24, *vol. IV.*

Substitute for Blistering Plaster.—The inconvenience often attending the application, for several hours of a common blistering plaster, is well known to us all. In children this difficulty occurs frequently; and in adults, where the functions of the brain or nerves are in certain states of derangement, it is almost impossible to keep such a plaster in close contact with the skin long enough to produce the desired effect. It is therefore with some degree of satisfaction we notice favorable accounts of the substitution, for such plasters, of the liquid preparation proposed by Sir Charles Scudamore. This preparation is a strong solution of cantharides in concentrated acetic acid. If the surface which we wish to vesicate be smeared with this composition about two or three minutes by a common varnishing brush, the blister will rise in about four or six hours, and the discharge of serum be as free and abundant as if the part had been affected by a common plaster of cantharides.

Parts to which the usual blister is inapplicable may be vesicated by means of this liquid, and, we doubt not, its obvious advantages would lead to its exclusive use, could we once break the ice, and get out of the *habit* of prescribing the ordinary plaster. At all events, it will be a service to the profession and the public, if physicians will make trial of this method, and send us some account of the result of their experience with it.—*Ibid. No.* 1, *vol. V.*

A certain remedy for the Tooth-ache. By Dr. Ryan. (*London Med. and Surg. Jour.*)—If the experience of others confirms that of Dr. Ryan, in the simple remedy here proposed for a most distressing malady, he will deserve a civic crown, in addition to the thanks of the profession.

"A remedy which is capable of affording immediate relief to the excruciating pain of tooth-ache, without the slightest pain being produced by its application, has long been a desideratum; and I feel great

gratification in being the medium of proposing such a valuable remedy to the profession.

It is right to observe, that before I resolved upon this course, I deemed it necessary to determine the value of this agent, and to try it upon myself and many other individuals; and ample experience has convinced me of its efficacy.

Like many of our best remedies, that which I proceed to notice was discovered by accident. A gentleman who attends my lectures (Mr. Myers of Newington Causeway,) had frequently applied sulphuric acid to his tooth with some relief; but on one occasion, he, in a moment of confusion, took down the next bottle to his remedy, which contained nitric acid. To his great surprise, he experienced immediate relief, and without the slightest pain. Since that period he has not suffered from tooth-ache, though three years have now elapsed. During the last winter he informed me of the success of this remedy, which induced me to try it, while labouring under the most intense pain from tooth-ache. The effect was immediate, and no pain whatever was induced. I have since used it in numerous cases, and invariably with complete success. In some instances the disease does not return for days or weeks; and in others not for months.

The best mode of employing it is by means of lint wrapped round a probe, and moistened with the acid, which is then to be slowly applied to the cavity of the tooth; care being taken not to touch the other teeth, the gums or the cheeks. On withdrawing the probe, and inquiring how the patient feels, the usual reply is, 'the pain is entirely gone.' The mouth is next to be washed with tepid water. The acid should be gradually applied to the whole cavity of the tooth, or otherwise a second application will be required before complete relief will be obtained.

This remedy may be used when the gum and cheek are inflamed so as to preclude the possibility of extraction. In cases where the diseased fang remains, and when the caries faces the adjacent tooth, it obviates the necessity of extraction in all cases of hollow teeth, which all practitioners declare to be desirable, if possible; and it enables the dentist to perform the operation of 'stopping or filling the teeth,' much sooner than he can otherwise accomplish. In a word, it will alleviate a vast deal of human suffering, and supersede a most painful operation. It is not a panacea for all the diseases of the teeth and gums, though a certain and efficacious remedy for the most common

causes of tooth-ache. It will be a valuable remedy for children, delicate persons, and pregnant women. It does not accelerate the decay of the tooth to which it is applied.

As the employment of this acid in the disease under notice is not recommended by any pharmacopœia, ancient or modern, of these or other countries with which I am acquainted, and as tooth-ache is now a most prevalent complaint, in consequence of the inclemency of the season, I think a more favourable opportunity cannot occur for the communication of the information described in this paper."—*Ibid. No.* 16, *vol. V.*

Catalepsy accidentally cured.—In the journal above mentioned, for October, 1830, there are related two cases of catalepsy, one in a female, the other in a male, which yielded to accidental hæmorrhage —in one case, from a cut on the head—in the other, from epistaxis. The first case was that of a young girl, ten years of age, who became cataleptic, the paroxysms returning at shorter and shorter intervals, and resisting all medicines. In one of these attacks the girl dashed her head against a sharp stone, when a profuse hæmorrhage instantly ensued. This hæmorrhage not only put an end to the attack, but prevented all returns of the disease.

The other case was that of a young farmer, twenty years of age, and of a melancholic temperament, who after some severe mental distress, became affected with cataleptic attacks, complicated frequently with delirium and somnambulism. Repeated bleedings, leeches, baths, blisters, and various other means, were employed in vain. At length a profuse nasal hæmorrhage took place spontaneously, and put an end to the attacks.—*Ibid.*

On the Use of Conium Maculatum in Affections of the Female Breast, and in Cancerous Ulcerations. By Stephen W. Williams, M. D. *late Professor of Medical Jurisprudence in the Berkshire Medical Institution.*—I beg leave to select for the Journal the following cases from my medical and surgical note book, in which I have long been in the habit of recording what I conceive to be important facts and cases.

Cancerous ulcerations.—Under this head I propose also to treat of the sequelæ of mammary abscesses, which, if neglected, frequently terminate in cancer. I am inclined to believe that a remedy will yet

be found for the cure of cancerous affections, and I think the faculty have underrated the powers of conium maculatum in these cases. This medicine was in high repute with the ancient physicians; probably by them it was too much extolled, and like many valuable medicines which were employed by them, it has, from this circumstance, gone into disrepute. Medicine, like other things, has its fashions. Within half a century, bleeding and powerful depletions have been in and out of fashion at least half a dozen times. Bleeding is now in vogue to a great extent, particularly in chronic affections of the lungs and liver. A reaction will probably soon take place, for, like all violent complaints, a crisis soon obtains. I believe in no *catholicon* in any complaint: what may be useful in one may not be in another, or even in the same in different individuals, owing to peculiarities of constitution. Yet I think that facts in favour of conium maculatum are multiplying. The following have occurred in my practice. I am not able to state them so minutely as I could wish, as many years have elapsed since some of them occurred, and I took no notice of them at the time.

Case I.—A woman from Hawley called upon me in the summer of 1820, with an indurated breast. Several months previously she had had a milk abscess, but I do not recollect whether it had ever been opened. At any rate the whole breast became completely indurated, and did not yield to any remedies her physician prescribed. At the time I saw her, the breast was large, the whole substance of it was indurated, unequal, and knotty, and all the integuments of it black. There were very slight constitutional symptoms. I directed the use of the white pond-lilly poultice, (Naphe* advena,) for a few days, hoping by the use of it to induce suppuration. After this, if any hardness remained, I prescribed the powdered leaves of conium, commonly called cicuta, and directed them to be sprinkled upon the poultice, and if they could not be obtained, I advised the use of a plaster of the extract of conium, and to continue the use of it as long as any hardness remained. I directed at the same time the internal use of the extract, beginning with small doses and gradually increasing until constitutional symptoms appeared. I am not able to give a detail of the progress of the cure, but I learnt that she followed my directions, and in the course of a few weeks was completely cured.

Case II.—Mrs. K. of Greenfield, soon after delivery, was severely afflicted with milk abscess, which suppurated, and was opened several

* We presume *Nuphar* advena is intended.

times. It was many weeks before the abscess healed. After it did heal, the whole of the breast became indurated, and as hard as stone. Various discutients, were applied, such as mercurial ointment, camphorated liniment and spirit, &c. &c. to no effect. I applied a plaster to her breast of the extract of conium, which she continued for several weeks, and recovered completely.

Case III.—Mrs. G. of Sunderland, had borne several children. Soon after the first one was born, she had a severe milk abscess. She was not able to suckle her child any more from that breast. With each succeeding child she had abscesses in the breast, which became more and more severe. The indurations which succeeded remained from one pregnancy to another. At last obstinate sinuses followed, which were laid open in all directions; these were followed with very troublesome funguses, which discharged a great quantity of fetid sanious matter, and yielded to no remedies which had heretofore been administered. A celebrated surgeon who attended upon her repeatedly, told her that there was no other possible chance of saving her life, than by removing the breast by the knife. She was pale and emaciated, and evidently rapidly declining. She was unwilling to submit to the operation, and sent for me. I found her as above described. I was inclined to believe that the surgeon was correct, and that the breast must be removed, but she was so anxious to try something else, that I consented to gratify her. I put her upon the use of the conium pill, and directed the use of the pond-lilly poultice, sprinkled with the powdered leaves of the conium. In a few days I saw her again. The matter discharged was much more bland, and less in quantity. It continued to decrease, and by the time that constitutional symptoms from the use of the pills manifested themselves, the sinuses were dried up. I then directed a plaster of the extract to her breast, which was worn till the indurations were completely removed. She recovered. She has since had children, but her breast has never troubled her any more.

(Case IV. was fatal; although the conium was resorted to.)

Case V.—Mrs. F. of Buckland, called upon my father and myself to consult us upon an affection of her breast, in the summer of 1827. She was between forty and fifty years of age, and her youngest child was about four years old. She had been troubled with milk abscesses with her former children. When young, her breast had been injured probably from pressure from the use of stays, a prolific source of can-

cer of the breast. The nipple was flattened, and she was never able to suckle from that breast. At the time we saw her the nipple was drawn in, and the breast around it was much puckered. It had been in that situation several months. The whole circumference of the areola was enlarged and tender. There had been an abscess under the lower part of the areola which had discharged and still continued to discharge a small quantity of thin matter. The opening would scarcely admit the head of a small probe. It occasionally scabbed over. In the upper part of the breast there was a scirrhous tumour, of a stony hardness, about the size of a butternut. It hurt her to press upon it. She was of the nervous temperament, and had been taking medicine for some time. When we saw her, her physician pronounced, with a great deal of confidence, that it was a case of real cancer, and said nothing but the knife would effect a cure. We prescribed for her the external and internal use of the extract of conium, and a continuance of the blue pill, which she was then taking. I saw her a fortnight after; the swelling around the areola had subsided; the scirrhus had also diminished, and the soreness had left her. The matter discharged was of better consistence. Directed to continue the remedies. Saw her again in another fortnight; she was under the full influence of the conium. She said it made her drunk. Her breast appeared, in every respect, better, and there was every prospect that she would soon recover, and I afterwards learnt that she did recover.

Case VI.—The following case shows that the conium has been successful in a case of *real open cancer*. In July, 1827, I took a short excursion upon our western mountains. On my journey I was requested by Dr. Deane, of Colrain, to visit a patient of his, Mrs. S. who was troubled with an affection of the breast. She had been confined to her room several weeks. On opening the door I most sensibly perceived that most disagreeable smell which is so peculiarly and characteristically attendant upon open cancer. I examined her breast. There was an open, deep, ragged ulcer upon it, nearly the size of the palm of my hand. It was discharging a thin, ichorous, and highly offensive matter, in large quantities, and occasionally blood. It had somewhat the smell of old brass or copper when exposed to heat and moisture. The edges were jagged and callous, and the whole breast was of a scirrhus hardness. It had been open several months, and there had been an induration in her breast several

years. Her constitution was much affected, and she was confined to her room, and the principal part of the time to her bed. The pain in her breast was intolerably severe. The stench was so great in her room that her attendants could not bear to stay with her. At the time I saw her she was taking large doses of laudanum to alleviate her distress. As I had no expectation of curing her, I advised Dr. Deane, merely for palliatives, to wash the breast freely with salt and water, to make use of a carrot poultice, sprinkled with powdered conium, and after the stench was removed, to use the conium plaster, and at the same time to put her upon the internal use of extract of conium, until constitutional symptoms were induced. On the 12th of October, I saw Dr. Deane, who informed me to my great surprise, that Mrs. S. had completely recovered, and that her breast was entirely healed, and as smooth as his hand. The next summer I was called to visit a patient in Colrain, and called on Mrs. S. and found her perfectly well.

I have since used the conium externally and internally with complete success in the case of Mr. H. N. of Greenfield, who for a number of months, had been affected with scrofulous indurations about the glands of his neck.—*Amer. Jour. Med. Sciences, No. XVII.*

Treatment of Chronic Dysentery by Sulphate of Copper. (*Lond. Med. Gaz. June,* 1831.)—It may be recollected that some years since Dr. Elliotson in a paper in the Medico-Chirurgical Transactions, recommended the sulphate of copper in the treatment of chronic dysentery. The following observations on this subject extracted from a clinical lecture given by him at St. Thomas's Hospital in March last will be read with interest.

"There was likewise presented a case of *chronic dysentery,* which exemplified the good effect of sulphate of copper united with opium. This man, like most of the patients that we take in here with chronic dysentery, I might say, perhaps, all, had been in a hot climate. He had several stools a day, and when he came in they were bloody. I began the sulphate of copper in doses of half a grain, three times a day, with half a grain of opium. These were gradually increased; but while I was employing them, there was no reason whatever for not having recourse to any antiphlogistic measures that might appear necessary.

"The present case was useful as showing a fact which is seen con-

tinually, respecting doses of sulphate of copper; namely, that the difference of half a grain may make all the difference in the benefit. This man took at last two grains, three times a day, with a certain benefit; but, not mending so fast as could be wished, the dose was increased to half a grain more; the result of which was, that he immediately began to improve rapidly. I believe I have mentioned before that it should be given in solid form, and not on an empty stomach; and that it is best combined with opium, at least in the first instance. I have frequently given it with two or three grains of opium, and at last have gradually diminished the opium till I left this off altogether. As, however, besides being an astringent, it is acrid, it is best to obviate the effect of the acrimony by opium. Of course, where it is given with opium, you cannot tell what are the effects of the sulphate of copper and what of the opium, because the opium itself has a strong tendency to check the diarrhœa. It is only from the comparison of a number of cases treated with opium and sulphate of copper, with cases where opium only was employed, and from cases where the opium has been greatly diminished and omitted, while the sulphate of copper was increased and continued, that the fact can be ascertained; and by comparing cases where opium was first given alone, and then the sulphate of copper added. It is only by these observations that its use can be proved. Of its good effects alone, I have no doubt; but knowing the advantage of opium, I consider it my duty to give a patient all the benefit that medicine will allow, and therefore I unite them together, provided the opium do not disagree.

"There is a case in the same ward at this moment, of chronic dysentery, which was very bad, but is now doing exceedingly well, and which also illustrates the benefit arising from the addition of half a grain only of the sulphate of copper. It has occurred in a young man who, I believe, has been at St. Helena. His stools continued bloody; when he came in, he had a great many in the day, and had been ill a year and a half; he had ten stools a day, sometimes twelve, sometimes fifteen. There was tenderness of the abdomen, and, therefore, to give astringents without attention to the inflammatory state, would have been wrong. Astringents for the diarrhœa were indicated, but still there was so much tenderness that I thought it right to apply leeches to the abdomen, and he had twenty applied from the 2d till the 15th, and then I began to give him half a grain of opium, and half a grain of sulphate of copper, which was gradually increased

till I came to two grains each; under which he continued improving, but not so rapidly as I desired. On the addition, however, of half a grain, he instantly began to mend very considerably. The last report is, that he had only one motion in the course of the twenty-four hours, and that of an healthy appearance. His stools before were liquid, and more or less bloody, but now they are generally healthy, and rarely show any blood."—*Ibid.*

Spontaneous Combustion. By Dr. Crockett.—The following case happened during the extreme cold weather of last winter, 1830–1. Mr. R. S. of Woodford county, Ky. having a stone jug of linseed oil to bring from Lexington in a waggon, in order to prevent its being broken, set it in and surrounded it with cotton flyings, in a barrel with but one head. Notwithstanding this precaution, the jug was broken, split into two pieces, and the oil ran into the cotton; but being packed close, it did not penetrate it far. This happened in the evening; next morning about 8 or 9 o'clock, the oil was squeezed out of the cotton, which was then laid up stairs in the dwelling house, where was neither fire nor fire-place, in a large piece of the broken jug. In about two hours, some of the family going up stairs, found the room into which it was put filled with smoke; and on examination the cotton was found consumed, all except a small quantity that was then in a state of ignition.

Although there seems to have been but little blaze, such was the heat, that the wall (of hewed hackberry logs) near which it was placed, was burnt in size about eight by ten inches, and in the middle about three quarters of an inch deep.

On the efficacy of large Injections, in the treatment of Bilious Colic. By J. S. Paige, *of Oswego, N. Y.*—I take the liberty of communicating the method of treating bilious colic which I adopted about twelve years ago, and which I have followed ever since, with the most complete success.

The plan is to use injections of mild, tepid fluids, to an amount sufficient to reach the obstructed or constricted portion of the gut, and thereby making a gentle, but firm pressure upon that part from below.

This practice was founded upon the incidents of the following case, which I will briefly notice. Mr. E. B. a strong, athletic man, aged

about 35 years, was attacked with this species of colic in the summer of 1818. I was called upon to attend him, and pursued the practice usually adopted in such cases. Among the means made use of, were venesection, opium, various cathartic medicines, enemata, fomentations to the abdomen, general warm bathing, a blister, &c. Forty-eight hours or more were spent in this way, and several consultations were had, but all to no purpose: the obstruction remaining as obstinate, and the pain as distressing as ever.

Another expedient presented itself to my mind, that of acting upon the obstructed portion of the intestine mechanically by injecting tepid fluids; six pints of warm water, milk and molasses, were ordered, which I commenced introducing with a syringe, intending to use the whole, or even more, if necessary, to effect my purpose. A consulting physician who was present, but unacquainted with my intention, requested me to desist, when I had used but a small part of the liquid; the patient also begged me to desist, saying that no more could be retained; but I paid little attention to the advice of my counsellor, or the entreaty of my patient, till the latter informed me that he felt a considerable degree of pressure upon the point of obstruction, and after retaining the fluid in this situation a few moments, I desisted, having used the whole quantity prepared, and I began to hope that my work was about to answer the purpose intended, which I found to be the case in about fifteen or twenty minutes. Copious evacuations of fetid matter followed in large quantities, and a cessation of pain, and a general mitigation of symptoms, were the immediate consequences.

The case was subsequently treated with mild laxatives, diaphoretics, &c; and after a proper time, gentle tonics were used to restore the action of the system.

Two or three weeks afterwards, the same person was again attacked in the same way from his own imprudence, and he found immediate relief in the same way, and in a like short time.

The success of this plan in these two cases afforded me a useful hint for the treatment of all important cases of a like kind, and I resolved to make further trials, whenever opportunities should present themselves; and a great many cases have since come under my care, all of which have been attended with the same result; and consequently, I have made this the standing rule or method of my practice for the last twelve years.

The rule I have adopted in such cases, and that I would recommend to others, is, to introduce a quantity of fluid with a large syringe, sufficient to make a firm, but gentle pressure upon the obstructed portion of the intestine, which may be known by the pain it gives the patient at that point; and more certain success may be expected, if the syringe be not withdrawn for a few minutes after this point be attained.

The quantity of fluid to be used, should be measured by the demand of each individual case; six pints usually answer the purpose; but in two instances, occurring in the same person, I have found it necessary to use twelve pints at a time, before the object was accomplished.

Perhaps it might be well to evacuate the rectum, before attempting to reach the obstruction. I think there would be an advantage in doing so, as some little impediment would be removed.

In the sequel, such treatment as the particular case shall require, will, of course suggest itself to any skilful physician, taking into view the cause of the disease, and other circumstances attendant upon the disease.

I generally give mild laxatives, such as castor oil, or neutral salts if the case be a simple one, brought on by exposure to cold, or some comparatively trifling circumstance; but if it depend upon some more obviously important cause, as an hepatic derangement, &c. other and more rigorous means should be used.

With regard to the composition of the fluid to be used in such cases, I should not be particular, provided it be sufficiently mild; perhaps mucilaginous injections in many instances might be preferable, particularly if the disease be of some considerable standing, and the intestines irritable or inflamed. It may, however, be proper in some cases to use stimulating mixtures, where there is much torpor of the bowels. But it is the mechanical effect that is almost entirely to be relied on, for success in this mode of treatment.

The reasons which urge me to recommend this mode of practice above all others with which I have been acquainted, are the following:—

1st. Because it is more certain; a mechanical dilation of the intestine in this way is easily effected, and in a short time.

2d. Because medicines given by the mouth are so liable to be rejected by vomiting in this disease; or if they are retained, they require

more time to act, and are not so sure as this, for they may fail from inefficiency.

3d. Because, although other medicines may act in their usual time, yet by delay the patient is endangered, by the risk of inflammation supervening.

4th. Because it abbreviates the term of human suffering, by mitigating some of the keenest pangs to which our nature is subject; &c. &c.

I give you only the outline of this plan, and if it shall be thought worthy of adopting in this species of disease, and extended to other affections of a somewhat similar kind, you, and those who may become acquainted with it, can make such improvements as shall appear most proper.

You may say, that the idea of introducing large quantities of liquid into the intestines, is not new; it certainly is not; but the application of this practice I have not known to be made to the disease of colic; but in cases of intus-susception of the gut, it has long been recommended and used.—*New York Med. Jour. No. III.*

Complete Suppression of Urine for seven weeks. (*Medico Chirurgical Review.*)—We notice this case, as published in a late number of Hufeland's journal, for reasons which will appear in the sequel.

Case. Itzig, a boy, 12 years of age, and who had always enjoyed good health, was taken, in the latter days of December, with a kind of malaise, which lasted five or six days, and for which he took some purgative medicine, which acted also as an emetic. From this date, the urinary secretion ceased entirely, not one drop of urine being passed, although the boy drank freely of liquids. Once only he passed about a pint of urine, after taking some diuretic medicines, and then the secretion stopped. Dr. Hubenthal was then consulted, and prescribed various medicines during a space of nineteen days, without any success. At the end of seven weeks, viz. on the 20th of Feb. Dr. Ramm visited the patient, and found that no urine had passed for many weeks; that a natural motion of the bowels took place every second or third day; that there was no increase of perspiration, but the reverse; that the apetite was tolerable, and sleep sound. The boy in all other respects appeared healthy. There was no reason to suspect any deception on the part of the boy or his parents. On the 22d Feb. there was a consultation of several physicians. The abdo-

men was rather distended, as there had been no stool for some days. A catheter was introduced into the bladder, but not a drop of water flowed. Injections of castor oil, with oil of turpentine, were thrown up per anum, and turpentine frictions were applied over the loins, and bals. copaivæ was ordered internally. These remedies were commenced on the evening of the 22d, and on the 24th, a few drops of urine mingled with blood were passed. In a few hours more, a quart of water was discharged, and this was repeated every day afterwards.

Most people will be sceptical as to the truth of the above narrative; and we should have doubted its authenticity ourselves, had we not lately seen a case where some weeks passed without any urine being secreted, and where it was impossible that any deceit could be practised. The catheter was introduced almost every day, but nothing except a few drops of blood followed. There was no affection of the head all this time; but a tumor existed in the right side of the abdomen, very painful, and apparently of a malignant nature. We this day (5th Jan. 1831) examined the patient, and ascertained that not a drop of urine had passed for five weeks! Mr. Acret can attest the truth of this assertion, strange as it may appear.

Since the above was written, the tumour burst internally—a large quantity of fluid was discharged per anum—and the patient is recovering.—*Ibid.*

OBITUARY NOTICE.

Died, in this place on the 1st inst. Mr William Ferril Baird, of Tennessee, in the 23d year of his age. The deceased was an amiable and respectable member of the Medical Class of Transylvania University, whose manly deportment and studious habits gave promise of great future usefulness in his profession. It will afford consolation to his bereaved relations to know that he received from the Medical Faculty during his fatal illness, and from his numerous friends who were around him, all the aid that professional skill or kindness could suggest, and that he manifested in his last moments, the composure and resignation of a Christian.—*Lexington Observer, January 6th*, 1832.

THE TRANSYLVANIA JOURNAL OF MEDICINE AND THE ASSOCIATE SCIENCES. EDITED BY LUNSFORD P. YANDELL, M. D. PROFESSOR OF CHEMISTRY IN TRANSYLVANIA UNIVERSITY, ASSISTED BY THE MEDICAL FACULTY.

THIS JOURNAL, emanating from the Transylvania School of Medicine, has now completed the fourth year of its publication. Its success has been satisfactory to those who have had charge of it, and the steady increase of its subscription is proof that it has not disappointed the expectations of its patrons. The experiment may be considered as having been fairly made, and no room is now left to doubt, that such a publication meets the wishes of the Medical Profession of the South and West, and that it will continue to receive adequate support. It remains to be shown how far the interests and value of its pages may be increased by the further efforts of Southern and Western Physicians.

The present Editor, in entering upon the duties of his station, is fully aware of the labour and responsibility which he incurs. In justice to himself he may add, that he would not have had the rashness to engage in the task, but for the pledge of the continued support of his colleagues. His predecessors while relinquishing their editorial charge, will continue their connexion with it as contributors to its pages. Its patrons may therefore rest satisfied, that although it has passed into other hands, it has not ceased to be the offspring of the Transylvania Medical School, and that there will be no relaxation on the part of all concerned to render it worthy of its extensive patronage.

The present plan of the Journal will not be materially changed, except, that in the absence of original articles, a large portion of it may be devoted to the review of new, interesting Medical Works. Practical utility will be kept constantly in view; and instituted as the work was, to promote Medical Science in the West, the Editor pledges himself to labour assiduously for the attainment of this end.

CONDITIONS.

1. The "*Transylvania Journal of Medicine and the Associate Sciences*," will be published *quarterly*, as heretofore, in octavo form; each number containing 150 pages, forming a volume of 600 pages in the course of the year.

2. It will be printed with a new type and on the best paper that can be procured in the Western country.

3. The first number of the 5th volume will be issued during the first quarter of the year, and regularly every three months thereafter.

4. The price to subscribers will be *three dollars* annually, payable in advance, or *four dollars* at the end of the year.

☞ All communications intended for this Journal should be addressed (*post paid*) to the Editor.

Lexington, December 23, 1831.

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